

THURSDAY, AUGUST 2, 1877

THE PHYSICAL BASIS OF MIND

The Physical Basis of Mind, with Illustrations, being the Second Series of Problems of Life and Mind. By George Henry Lewes. (Trübner and Co., 1877.)

WHEN the first volume of "Problems of Life and Mind" appeared, I ventured to say that perhaps Mr. Lewes promised too much in undertaking to exhibit "how the sentient phenomena may be explained by neural phenomena." I also directed a criticism, as pointed out as I could make it, against a proposition placed by Mr. Lewes at the foundation of his psychology; namely, that "actions are prompted and really guided by feeling." The present volume is, in addition to much else, Mr. Lewes' fulfilment of his promise and his reply to the criticism.

Considering the limited amount of space at my disposal, I shall, I believe, put it to most advantage by confining myself to these two points. As regards the first—the possibility of finding a physical basis of mind—a sentence in the preface rouses misgivings: "Materialism, in attempting to deduce the mental from the physical, puts into the conclusion what the very terms have excluded from the premisses;" "the attempt to interpret the one by the other" is a legitimate undertaking only "on the hypothesis of a physical process being only the objective aspect of a mental process." This is of ill omen; that which can be done as science does not seek its justification in metaphysics. But let the interpretation be taken on its merits. What is it? Though prepared for disappointment, readers will doubtless be surprised to hear that when looked for, it is nowhere to be found. "The sensation, or state of consciousness," says Mr. Lewes, "is the ultimate fact; we can only *explain* it by describing its objective conditions." In place of the second proposition, "we can only *explain*," &c., most thinkers prefer to say we *cannot* explain it, we can only describe its objective conditions. The difference, then, between Mr. Lewes and others, is not that he has any new light to offer, but that he insists on calling that an explanation which others cannot see to have that character. The sense in which Mr. Lewes thinks he can correctly call a description of neural processes an interpretation of mental facts rests on his statement of the metaphysical hypothesis that these are but "different aspects," "the two faces of one and the same reality." "It is thus indifferent," he continues, "whether we say a sensation is a neural process or a mental process; a molecular change in the nervous system, or a change in feeling." Suppose all this to be understood and granted, where is the explanation or interpretation of the one by the other? Is a description of one aspect of a thing an explanation of a very different aspect of the same reality? Not even metaphysical legerdemain can give the illusion of a physical basis of mind. Mr. Lewes sees that it is impossible to conceive a neural process as *causing* the mental process. He does not say that molecular movement becomes, or is transformed—in any sense, conceivable or inconceivable—into sensation. Mind is not the

outcome of physical conditions or combinations; it is an *aspect*, "the spiritual aspect of the material organism." Readers may now judge whether Mr. Lewes can claim to have explained sentient phenomena by neural phenomena, to have shown the manner in which the Self and Not-self "are combined in feeling and thought."

Against Mr. Lewes' proposition that the movements of living beings are prompted and guided by feeling, I urged that science has carried us to a point at which we have but to pause and reflect to see that all movements must be the consequents of purely physical antecedents; that the amount and direction of every nervous discharge must depend solely on physical conditions. And I contended that to see this clearly is to see that when we speak of movement being guided by feeling, we use the language of a less advanced stage of enlightenment. This view has since occupied a good deal of public attention. Under the name of Automatism it has been advocated by Prof. Huxley, and with a firmer logic by Prof. Clifford. It has been argued about in the *Spectator*, zealously combated by Dr. Carpenter, and now Mr. Lewes makes it the subject of one of his Problems, devoting seven chapters to its discussion.

Mr. Lewes cannot think that Prof. Huxley really holds the repulsive doctrine in question, though "supposed to hold (it) by those whom his expressions mislead." Yet, curiously enough, it is against Prof. Huxley's statement that Mr. Lewes' polemic is specially addressed. It is not my affair to reply for Prof. Huxley. Mr. Lewes has, however, mentioned me as having insisted "with iterated emphasis" on the view he now "most earnestly desires to refute." I must give my own statement. Here it is as given in my review of "Problems of Life and Mind" (*The Examiner*, March 14, 1874):—"Using the word feeling in its ordinary acceptation, as a name for subjective phenomena alone, we assert not only that no evidence can be given that *feeling* ever does prompt or guide action, but that the process of its doing so is inconceivable. How can we picture to ourselves a state of consciousness putting in motion any particle of matter, large or small? for this is really what it comes to. . . . Puss, while dozing before the fire, hears a slight rustle in the corner, and darts towards the spot. What has happened? Certain sound-waves have reached the ear, a series of physical changes have taken place within the organism, special groups of muscles have been brought into play, and the body of the cat has changed its position on the floor. Is it asserted that this chain of physical changes is not, at all points, complete and sufficient within itself? Mr. Lewes, we believe, will not assert this; he will admit that the material succession is unbroken. Once more, then, in what sense can we take the proposition that actions are prompted and really guided by *feeling*?" Putting in the place of my cat hunting for a mouse, the analogous case of a wolf springing on a sheep, Mr. Lewes replies: "Unless the term *physical* is here used to designate the *objective sequence*, as contemplated by an onlooker, who likens the process to the sequence observable in a machine, I should say that from first to last the process has been *not* physical, but *vital*." The word "unless," with which the reply opens, might be objected to, as implying that the term "physical" might be here employed to designate something else than the objective sequence—that succes-

sion of external events which can be seen or imagined in terms of vision. Quite irrelevantly, as it seems to me, Mr. Lewes specifies a particular kind of on-looker—one who likens the process to the sequence observable in a machine. I will only say that for myself I decline the honour of a place among those physiologists and philosophers who, according to Mr. Lewes, have failed to perceive the "radical difference between organic and inorganic mechanisms." However, Mr. Lewes has put it on record that *if* when I spoke of a series of physical changes taking place within the organism I meant series of *inorganic* changes—that the movements of the cat resulted from something of the nature of a combination of levers, springs, and pulleys, then, he "should say that from first to last the process has been *not* physical but *vital*." And who will question that Mr. Lewes would be quite right in so saying? But why suppose anything so unlikely? Yet this is the meaning Mr. Lewes gives to the word "physical" when it occurs in the mouths of those against whom he directs his arguments. For instance: physiologists are in the habit of describing unconscious reflex movements as physical processes. Of this description Mr. Lewes says: "Restate the conclusion in different terms and its fallacy emerges; 'organic processes suddenly cease to be organic, and become purely physical by a slight change in their relative position in the consensus.'" But to proceed. Not having used the word "physical" in any peculiar sense, but in accordance with ordinary usage, my question remains—"Is it asserted that the chain of physical changes is not, at all points, complete and sufficient within itself?" So far is Mr. Lewes from denying the physical succession to be unbroken, that he states this, or something very like it, over and over again, as a truth almost too self-evident to require expression. Thus we read: "So long as we are dealing with the objective aspect we have nothing but material processes in a material mechanism before us. A change within the organism is caused by a neural stimulation, and the resulting action is a reflex on the muscles. Here there is simply a transference of motion by a material mechanism. There is in this no evidence of a subjective agency; there could be none." But we also find statements that seem to have a contrary implication. Here is one: "The physiologist is compelled to complete his objective observations by subjective suggestions; compelled to add feeling to the terms of matter and motion, in spite of the radical diversity of their aspects." How is he *compelled* to infer that of which Mr. Lewes has just told us there could be "no evidence"? Again, while the volume abounds with detailed descriptions of the behaviour of dogs, frogs, and men, given as instances in which it is "evident enough," to Mr. Lewes, that their actions were "determined by sensations, emotions, and ideas," yet Mr. Lewes is equally positive that we are "passing out of the region of physiology when we speak of feeling determining action. Motion may determine Motion, but Feeling can only determine Feeling." Where, then, are we, when we talk of feeling determining action? In, I maintain, the gray morning of that intellectual light which is still far from having reached its noon-day splendour.

In the minds of our savage ancestors *feeling* was the source of all movement. Every one of them had what Mr.

Lewes, after all he has written about scientific method, can call "the irresistible evidence each man carries in his own consciousness, that his actions are frequently—even if not always—determined by feelings;" and they spoke according to their light. But while we shall continue to speak of feeling determining action, it will only be as we speak of the rising and the setting of the sun. Mr. Lewes is of a different opinion. He says: "We do so speak and are justified. For thereby we implicitly declare, what psychology explicitly teaches, namely, that these two widely different aspects, objective and subjective, are but the two faces of one and the same reality." If Mr. Lewes did not go farther than this I should not care to quarrel with his endeavour to put a new metaphysical meaning into the language of old error. But he thinks that on the strength of this hypothesis the material succession may be regarded as unbroken, and yet a rational interpretation found for the proposition—actions are prompted and really guided by feeling. Because the molecular changes in the brain which form part of the series of material changes involved in the production of motion may be held to be, in a metaphysical sense, the other side of what we know as feeling, Mr. Lewes somehow concludes that "we must declare consciousness to be an agent (in the production of motion), *in the same sense that we declare one change in the organism to be an agent in some other change*" (the italics are by the author). Let us see. The word "consciousness" here denotes two things assumed by Mr. Lewes to be two faces of one thing. If we substitute for this word one of these denotations and say "we must declare the molecular changes involved in the production of motion to be an agent, &c.," the statement becomes the most empty tautology. If we give to the word "consciousness" its other meaning—*feeling*—the proposition becomes what Prof. Clifford calls "nonsense;" and is, as Mr. Lewes says, placing feeling where "there is obviously no place for it—among material agencies." If by "consciousness" Mr. Lewes means neither the molecular changes nor the feeling, but the something of which both are but aspects, then he is altogether beyond science, and for the moment it is enough to say that this metaphysical entity is *not* an agent "*in the same sense*," &c.

Corresponding to those feelings, which Mr. Lewes will have it inspire and guide movement, there are conditions of the organism which can be conceived as the causal antecedents of the movements—the feelings, as admitted, cannot. Our instinctive faith in the unity and constancy of things leaves us no room to doubt that identical organic conditions will ever be accompanied by identical feelings and followed by identical movements; but this does not bring into view any scientific sense in which the feelings can be said to inspire and guide the movements. These for ever remain parts of an infinite series of physical consequents following on physical antecedents. This is the thesis at present so repulsive to many minds. Against this Mr. Lewes has nothing to advance. If any look to him for comfort they will find that, promising them bread, he gives them a stone—the same stone that has already set their teeth on edge.

One word to correct a false impression that the foregoing critical remarks would leave on minds unacquainted with Mr. Lewes' writings. Let no one suppose that I have

not read the book with admiration. Like all Mr. Lewes' works, it is a repertory of suggestive fact and of equally valuable and suggestive thought; and if any reader derive from its perusal a tithe of the intellectual stimulation it has afforded me, he may regard his time as well spent. Reflective minds are diligently working towards clearer conceptions in a region that has hitherto been all obscurity. There is reason to believe that ere long philosophic thinkers of the highest rank will for the first time agree as to one or two fundamental conceptions. Few living men have done as much as Mr. Lewes to usher in this new era. Knowing my criticisms to be inspired solely by the same impersonal motives by which he has himself been sustained throughout his extensive labours, I am sure Mr. Lewes would be the last person to suggest that I could have made better use of the space at my disposal. Others, better qualified than myself, will draw attention to the importance of those parts of the work that I have not mentioned, as, for instance, the splendid essay on the Nervous Mechanism.

DOUGLAS A. SPALDING

GORE'S "ELECTRO-METALLURGY"

The Art of Electro-Metallurgy; including all known Processes of Electro-deposition. By G. Gore, LL.D., F.R.S. Text-books of Science Series. (London: Longmans, Green, and Co., 1877.)

DR. GORE has evidently spared no pains to make this text-book a complete manual of the art of electro-metallurgy. Beginning with the history of the subject, he gives an interesting account of the rise and development of the art, full of names and dates and references, and makes the early inventors tell, as far as may be, their own story by quoting freely from their published papers. Then comes a "theoretical division," about which we have something to say presently, and this is followed by what forms the greater part of the work—a detailed account of practical methods of depositing the various metals. This portion of the book, at once thoroughly circumstantial and comprehensive, cannot fail to prove most useful to the practical electroplater as well as to the scientific student. The metals most commonly employed in the arts receive, of course, most attention; but almost none, even of the rarest metals, pass without notice, and the experiments are described with the precision that comes only of experience. An admirable feature of Dr. Gore's book is the habit he has of giving specific references to the authorities he makes use of, so that any one with a library at his command may, if he choose, turn up the passages cited. The remainder of the book is filled by a "special technical section" containing various practical directions and details, and, in conclusion, we have a list of the books previously published on the subject and of the English patents referring to electro-metallurgy. The author is to be congratulated on the accumulation and systematic arrangement of an immense mass of information of a kind that will be welcomed alike in the workshop and in the laboratory.

If Dr. Gore had given us only the practical parts of his book we should have had little to say beyond praise and thanks. Unluckily, however, for himself as well as for

his readers, he has introduced a chapter on the theoretical principles which underlie the art of electro-deposition. Such theoretical *réchauffés* are often to be found in practical text-books, but their existence is surely a thing to be protested against even when they are tolerably well written. No one can hope to give a satisfactory account of chemical and electrical theory in fifty pages, and when his work is to form one of a series in which chemistry and electricity have already been treated of in separate books, the attempt is not only useless but unnecessary. These short abstracts are certainly not to be recommended to the novice; and to the student who has already studied the subjects at greater length they are little short of an impertinence. In a book which stands by itself they might be tolerated if they were at once concise and accurate, giving what is needed and no more. In the case before us these extenuating circumstances are all absent. That Dr. Gore's "theoretical division" is not concise the following quotation will suffice to show:—

"The strength of the current is equal to the electromotive force divided by the resistance; this is known as Ohm's law; it is directly proportional to the electromotive force, and inversely proportional to the resistance; if the resistance remains the same, and the electromotive force varies, the strength is directly proportional to the electromotive force; and if the electromotive force remains the same, and the resistance varies, it is inversely proportional to the whole of the resistance in the circuit" (p. 71).

As an instance of matter which might very well have been left out, take the following. After giving a table of conductivities, Dr. Gore proceeds:—

"If the conduction-resistance of distilled water is so great in relation to that of copper, we can easily understand, by referring to the previous table, that the resistance of gases must be enormous. The electric conduction-resistance of air heated to redness (*sic*) is 30,000 greater than that of water, containing a 20,000th part of its weight of sulphate of copper in solution" (p. 31).

Why this long-buried result of E. Becquerel's (here, by the way, the authority is not cited) should be unearthed for the benefit of students of electro-metallurgy is almost as puzzling as is the strange piece of *à priori* reasoning in the first sentence, which, it is distressing to find, we are expected to understand easily.

The vagueness and inaccuracy of some parts beggar criticism, and leave the reviewer but one weapon—a severe one indeed, but he has no other—he can only quote. Here are a few specimens chosen almost at random.

"The fundamental act or principle of magneto-electric action is, wherever there is varying magnetism, there is an electric current induced in an adjacent closed circuit at right angles to it" (p. 57); the italics are the author's.

"The electromotive force, or strength of the current to overcome resistance, depends upon the degree of difference of strength of chemical affinity of the two metals for the electro-negative constituents of the liquid" (p. 70).

"The electromotive force (commonly called 'the intensity') of the current . . ." (p. 337).

"As the electromotive force is diminished by resistance, a diminution of resistance in any part of the circuit will increase it" (p. 337); this extract we have ventured to italicise.

"Motion of the articles is very advantageous . . . it

greatly diminishes the electric conduction-resistance which would be produced by polarisation, due to layers of liquid of opposite electrical nature, collecting in contact with the electrodes" (p. 344).

"*Potential and tension.*—Previous to the completion of the circuit and formation of an unimpeded current, the free ends of the polar wires attached to the two metals are charged with the two kinds of electricity in an accumulated or free static condition, and are in a state of *electric potential*, i.e., possessing a capability of doing electric work. These accumulated electricities in the wires may be detected by means of a very delicate electroscope. The free electricities are also in a state of *tension*, constantly tending to escape and unite; and their degrees of tension may be measured by means of an electrometer" (p. 71).

From which it would appear that the difference between potential and tension lies in the fact that the one is to be detected by an electroscope, and the other measured by an electrometer. It would be just as satisfactory a distinction, and would besides have the merit of being true, to say that "potential" is the shibboleth of the electrically unlearned, while "tension" is their refuge at all times.

Over and over again we find such phrases as these:—"If the current to be measured is one of low electromotive force" (p. 73); "a current of less quantity and greater electromotive force" (p. 338); and after we have been expressly told on p. 72 that there is no difference between currents except as regards their quantity per minute, it is surprising to learn that "as a general rule, the greater the electromotive force, and the smaller the quantity of the current, the harder and brighter is the deposited metal" (p. 344).

But it is needless to multiply examples. We have given enough to show how much Dr. Gore has done to mar a really good book by adopting a precedent which, however well followed, is of very doubtful utility. In the present instance it may, perhaps, serve the good purpose of acting as a warning to future practical writers.

We have noticed comparatively few typographical errors. Is it the author, or the printer, or the author's evil genius, or the printer's devil that we have to thank for this bewildering statement on p. 182?—

"Silver may be cleaned in water in which potatoes have been boiled, and a superior polish is thus imparted to them."

OUR BOOK SHELF

Enumeración de las Plantas Europeas que se hallan como silvestres en la Provincia de Buenos Aires y en Patagonia. Por Carlos Berg. (Buenos Aires, 1877.)

THIS is a very interesting list of European plants introduced by various means into the two above-mentioned countries. It gives the relative abundance of each species and the conditions under which it is found. Altogether 116 Dicotyledons, 30 Monocotyledons, and 8 Cryptogams are mentioned. Of these no less than 108 are common to Britain. As might be expected, the natural orders Compositæ and Gramineæ, each with 20 species, and Caryophyllæ with 12, are the strongest in point of number of species. Many notes are scattered through the twenty-four pages, from which we learn that under such extremely different conditions some of our British plants attain extraordinary dimensions.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Optical Spectroscopy of the Red End of the Solar Spectrum

BEING now, with my wife, on the return voyage from a private spectroscopic experiment on solar light in Lisbon, there appear to be two or three reasons why I should request your leave to send this preliminary note to NATURE before attempting to publish anywhere a full account of what was seen.

As the first of these reasons, I may mention that the continual assistance kindly afforded us by M. Oom, the Astronomer-Royal of Portugal, and the several facilities obligingly granted to us, through his intervention, by the Portuguese Government, render an early and hearty acknowledgment imperative. All the more so, too, as the last and most successful series of observations, through four successive days of blazing sunshine, without the smallest speck or suspicion of a cloud anywhere from morning to evening each day, was made in a new suite of rooms recently prepared for the local astronomer's residence in the Royal Park of the Ajuda.

The second reason is the pleasing one to confess, that out of four prismatic arrangements tried in the same spectroscope—the one which had the highest dispersion (viz., 32° from A to H) gave also the best and most satisfactory definition, showing thereby such wondrously fine and minute detail amongst close lines as to cause it to be almost invariably employed, and that prismatic arrangement, I am happy to say, was lately made for me by Mr. Adam Hilger, of 192, Tottenham Court Road, London, on his own long-approved plan of three powerful and symmetrical compound prisms, while the eye-piece of the telescope, also by him, was of rock-crystal, and fitted with his peculiar reference line for micrometer mensuration.

The third reason is the total contradiction given by the best of these observations to some conspicuous features of the Royal Society's last publication on the red end of the solar spectrum, when seen at a high altitude with their second and most improved "Indian Spectroscope."

Our late Lisbon measures, though made at a station close to the sea-level, were yet, near the noon of each day there, and on a midsummer sun in that latitude, taken through almost the very same thickness of atmosphere, as the Royal Society's, and Mr. Hennessey's high-sun series on the Himalaya Mountains. But those Indian observations having been printed in the *Philosophical Transactions* so long ago as 1874, I should be glad to know whether either the Royal Society or anyone else has published further particulars of the extreme red end of the solar optical spectrum since then.

PIAZZI SMYTH,
Astronomer-Royal for Scotland

The Cretaceous Flora of America

NEAR the close of his very interesting lecture "On the Tropical Forests of Hampshire," published in NATURE (vol. xv. pp. 229, 258, 279) Mr. J. S. Gardner says:—"I have great doubts, however, as to the correct position of many of the foreign so-called cretaceous beds. Those of America, from which most of the list of dicotyledons of this period is derived, appear to me, from the character of their fauna, to be rather lower eocene, or at most, filling in the gap between our chalk and London clay. Most of the shells have a marvellously eocene-like aspect, and I take it that the presence of an ammonite, and some few other forms of shells which in England do not reach above the chalk, should not be taken as conclusive evidence of the antiquity of the bed, as, although migrated from our seas, they may very well have lived on in other regions. It is inconsistent to assume that no ammonite lived on in any part of the world to a more recent period than that of our chalk."

From these remarks it is evident that Mr. Gardner is not fully informed in regard to the evidence which exists on the question he has raised; and as the subject is one of great interest, and one which it is necessary should be carefully understood by those

who write upon the progress of plant life on the globe, I take the liberty of reporting briefly what we really know in regard to the cretaceous flora of the North American continent.

Some twenty years ago numerous impressions of angiospermous leaves were brought by Dr. Hayden and myself from the group of sandstones which lie at the base of our cretaceous system. Outline sketches of a part of these were sent by Mr. Meek to Prof. Heer, of Zurich. He pronounced them miocene tertiary. To this conclusion he was led by their high botanical rank, their generic affinities with miocene plants, and the supposed identity of some of them with miocene species.

The announcement of Prof. Heer's decision led to a somewhat earnest discussion, in which Prof. Heer, M. J. Marcou, and Mr. Leo Lesquereux supported the view that the plants in question were tertiary, while Messrs. Meek and myself asserted that they were cretaceous, because the strata which contain them are overlain by more than 2,000 feet of limestones filled with characteristic cretaceous fossils, a number of which are identical with those found in the gault and chalk of Europe. An end was finally put to this debate by M. Marcou and Prof. Capellini, of Bologna, going to Kansas and collecting a large number of these leaves from beds overlain by unmistakable cretaceous strata. The true position of this flora was then not only acknowledged but proclaimed by these gentlemen, and since that time every geologist in America has accepted the statement which I made in my letter to Messrs. Meek and Hayden in 1858, and in my article on the Ancient Vegetation of North America (*American Journal of Science*, vol. xxix, 1860, p. 208), that "the American flora assumed nearly the botanical character it now has in the cretaceous age, and that our lower cretaceous rocks contain the remains of sixty or seventy species of angiospermous trees, many of which belong to our most common living genera, such as *Quercus*, *Salix*, *Magnolia*, *Platanus*, *Liriodendron*, *Fagus*, *Alnus*, *Liquidambar*, &c."

Since the settlement of this question a large number of additions have been made to the then known species of this flora, and it is probably not too much to say that we have obtained leaves of nearly one hundred species of angiospermous trees from the base of our cretaceous system, the equivalent of the upper greensand of England.

All the leaves figured in Lesquereux's "Fossil Flora of the Western Territories," part I., were obtained from this horizon, and a large number of additional species have been described by Prof. Heer in his "Phyllites Cretacea," or by myself in "Our Later Extinct Floras," while many others yet wait publication.

The plants of our upper cretaceous and tertiary rocks have not yet been fully described, and there is some difference of opinion as to where the line should be drawn between these two systems, but it is quite certain that a large part of the species described by Mr. Lesquereux from the "lignite beds," and referred by him to the tertiary, are really cretaceous; not only because they are associated with *Ammonites*, *Inoceramus*, and other cretaceous fossils, but because the strata which contain them underlie unconformably the *Coryphodon* beds, the base of our eocene. Whatever shall be ultimately decided in regard to the line of separation between our later cretaceous and earlier tertiary strata, this will in no wise affect our conclusions in regard to the general facies of the American cretaceous flora. The statements made many years since are confirmed by all fresh evidence, and now stand unquestionable, that between the trias and the chalk—we know nothing of our Jurassic flora—the vegetation of North America was revolutionised, and that at the beginning of our cretaceous age it had assumed essentially the character and consisted chiefly of the same generic elements that it exhibits now.

I may also add that up to the present time no species of *Ammonites*, *Baculites*, or *Inoceramus* have yet been found in America above the cretaceous system; and that so far as we now know, these genera are as decisive of the age of the strata which contain them here as in the Old World. J. S. NEWBERRY

Columbia College, New York, June 19

Meteorological Notes from Lisbon

THE following meteorological notes, compiled in great part from the daily bulletins of the Observatorio Real of Lisbon, supplemented by observations made by myself, by means of a Casella's self-registering thermometer and a good aneroid barometer, during a seven months' residence in that city, may not be without some value to weather observers. I arrived on

October 15, consequently the observations for this month refer only to the latter half. The records were made at 9 A.M. and at 5 P.M. To save space the readings will be given throughout (except for October) in the following order:—I. Barometer (reduced to sea-level), (a) the average of observations taken at 9 A.M., (b) the highest, and (c) the lowest reading of the month. II. Thermometer (Fahrenheit), (a) average of daily observations made at 9 A.M., (b) average of the highest, and (c) of the lowest readings in the twenty-four hours; (d) the highest, and (e) the lowest reading of the month. III. Direction of Wind: N. S. E. W. represent the directions indicated, or any point thereof, after which the number of days is given on which it blew from that quarter. IV. The rain of the month is stated in inches.

OCTOBER, 1876.—The morning temperatures ranged from 54° F. to 70°; midday, from 62° to 80°; and evening, 53° to 72°; the average of the night temperature for the half-month, 52°; and the average rainfall for the same period was 3.8 inches. No wind record was kept.

NOVEMBER, 1876.—I. (a) 29.95, (b) 30.44, (c) 29.44. This last reading is the record of the 12th, and was accompanied by a terrific gale from the south-west, which wrought much damage both on land, on the river, and at sea. Several residents, who were not unfamiliar with earthquake shocks, averred that they felt a distinct tremor of the earth about 4 A.M., at which time the barometer registered 29 inches. In the Bay of Biscay on the same morning the lowest point reached by the mercury was 28.25, as I was, I believe accurately, informed by the captain of a Glasgow steamer which arrived in the Tagus some days later. II. (a) 57.59, (b) 63.9, (c) 54.09, (d) 70.98, (e) 46. III. N. 7 days, S. 10, E. 7, W. 2; of 3 days no record. IV. Rain, 10 inches, which fell on 17 days. This was one of the most rainy Novembers for many years. The rainfall of the year 1874 was 17.2, and that of 1875, 18.3 inches. The total amounts for the months of November from 1873–1875 was 5.5 inches. The mean of this month for the last twenty years is 4.3 inches. Most destructive floods occurred during the month.

DECEMBER, 1876.—I. (a) 29.96, (b) 30.3, (c) 29.4 inches. II. (a) 54.8, (b) 59.5, (c) 51.6, (d) 65.5, (e) 44.2. III. N. 5, S. 18, E. 0, W. 5, calm 3 days. IV. Rain 19.19 inches on 28 days, greatest fall on 1 day (6th) 3.2 inches, and least .003.

JANUARY, 1877.—I. (a) 30.18, (b) 30.58, (c) 29.54 inches. II. (a) 52.93, (b) 58.96, (c) 50.8, (d) 65.66, (e) 44. III. N. 17, S. 9, E. 0, W. 3, calm 2 days. IV. Rain which fell on 14 days, 7.007 inches; from 1st to 10th, 6.669 inches.

FEBRUARY, 1877.—I. (a) 30.35, (b) 30.54, (c) 29.92 inches. II. (a) 52, (b) 56.64, (c) 48.29, (d) 67.38, (e) 42.9 (the lowest temperature of the seven months). III. N. 25, S. 1, E. 1, W. 0, calm 1 day. IV. Rain, which fell on 2 days, 1.28 inches.

MARCH, 1877.—I. (a) 29.94, (b) 30.39, (c) 29.36 inches. II. (a) 47.63, (b) 59.34, (c) 48.5, (d) 71.6, (e) 43.3. III. N. 10, S. 9, E. 1, W. 6, calm 1, of 4 days no record. IV. Rain, which fell on 13 days, 2.5 inches.

APRIL, 1877.—I. (a) 29.92, (b) 30.13, (c) 29.60 inches. II. (a) 65.5, (b) 62.9, (c) 51.9, (d) 71.7, (e) 48.2. III. N. 8, S. 8, E. 1, W. 11, 2 days unrecorded. IV. Rain in 17 days, 6.5.

I would draw the attention of those threatened with bronchial or pulmonary complaints to this locality as a winter and spring refuge. The site of the city of Lisbon is finely chosen, facing almost due south, and the position of the principal part of the town in which the chief hotels are, is nearly sheltered from the northerly and easterly winds by surrounding heights. It is of easy access from England—3½ days, and sometimes fewer, from Southampton by a royal mail steamer. Fires are rarely to be seen in a Portuguese sitting-room, and during the seven months of my sojourn there it was necessary only once or twice to have one in our room for an invalid's sake. I had an opportunity of seeing many sufferers both *en route* for, and again returning to England from, Madeira. Some of them complained much of the weather experienced there, and said how they wished they had remained in Lisbon, where the climate seemed equally to suit them, and where they should have had at least more comforts, more cheerful society, and more varied means for killing the Enemy—time. HENRY O. FORBES

Fertilisation of Flowers by Insects

IN my last article on Alpine *Gentiana* species, I supposed that the chief, if not the only fertiliser of *G. bavarica* and *verna* might be *Macroglossa stellatarum* with its proboscis of 25.28 mm.

length. Yesterday, near the Albula pass, I was happy enough to confirm this supposition by direct observation. Altogether I saw five specimens of *Macroglossa stellatarum* at work, one on *Gentiana bavarica* and *verna*, three on *Primula integrifolia*, and one on *Viola calcarata*, each of them in a few minutes fertilising some hundreds of flowers. For instance, the last of my five *Macroglossa* specimens, which I observed with the watch in my hand, in less than four minutes visited 108, and in other 6½ minutes 194 flowers of *V. calcarata*.

As an illustration to what I have said in a former article on alpine orchids generally being adapted to cross-fertilisation by Lepidoptera, I may mention that near my present domicile there grow nine species of orchids, eight of which (*Nigritella Augustifolia*, *Platanthera bifolia*, *Gymnadenis conopsea*, *odoratissima*, *albida*, *Habenaria viridis*, *Orchis globosa*, and *ustulata*) are adapted to cross-fertilisation by Lepidoptera, whilst only a single one (*Orchis latifolia*) is adapted to cross-fertilisation by other insects.

HERMANN MÜLLER

Wissenstein, Albula Valley, Switzerland, July 23

Local Museums

I HAVE read with very great interest both the letters and articles which have lately appeared in NATURE on the subject of local museums. The suggestions offered by your various correspondents are in every way admirable, and my only excuse for adding my own name to the number is because I think that although a great deal has been said on the matter next to nothing has actually been done. If local museums are to be established amongst us as a means of promoting advancement in education the sooner the matter is taken in hand by those most competent to deal with it the better.

What I would strongly advocate is that a society be formed in London for the promotion of local museums. If Prof. Boyd Dawkins, and any others possessing the requisite attainments for taking the matter in hand, would form an association of this kind, I, for one, and doubtless many others of your readers, would gladly subscribe and co-operate for the realisation of the scheme.

J. ROMILLY ALLEN

34, James Street, S.W.

Proposed New Museum

Now that the new Natural History Museum is approaching completion, will you allow me to call attention to a need which has probably been felt by others beside myself, and which we may hope will be met in the new institution? This is a museum or collection of varieties of plants and animals produced by domestication. I need not enlarge upon the value of such a collection to the student of biology. The revolution in the philosophy of biology created by Mr. Darwin was founded upon an examination of such varieties, and I have small doubt that my plea will be seconded by botanists and zoologists who will speak with much greater authority than I can.

I base my own request upon another ground, and one which touches very closely the science I am chiefly conversant with, namely, ethnology. Rutimeyer in Switzerland, Busk, Dawkins, and others in England, Brandt in Russia, and others elsewhere, have shown how invaluable the evidence furnished by varieties of domestic animals is for elucidating the earlier history of our race. Yet there is no collection known to me anywhere except the one made by Mr. Darwin himself, illustrating the subject, and if one wishes to examine the various breed of cattle, sheep, dogs, or pigs, of vegetables and fruits, &c., which have become localised in various parts of the world, as the companions of man, one is entirely at a loss for materials in an accessible form.

May we hope that the very efficient staff of the National Museum will see their way to setting apart one room at least in which the variation of animals and plants under domestication may be shown, and the glorious discoveries of the greatest biologist of modern times may be fitly illustrated in the National Museum of the country whose science he has so adorned.

HENRY H. HOWORTH

Adaptation of Plant Structure

I HAVE lately observed a curious adaptation of plant-structure which has not, to my knowledge, been recorded in books, and which may be interesting to your botanical readers.

There is in the Himalayas an *Arum* bearing a remarkable resemblance to a cobra with its hood raised, which is well

known to natives and many Europeans by the name of the "cobra plant." Standing immediately behind and above the spathe is a large ternate leaf, the two lower leaflets of which, at an early stage of growth, enfold the spathe and spadix, and subsequently stand in front of and partially conceal them from view. When, however, the anthers or the stigmas, as the case may be (for the plant is dioecious), are mature, the lower halves of these lateral leaflets fold close up over their upper halves, thus leaving the whole of the spathe conspicuously exposed to the notice of passing insects. I inclose a rough sketch made from a living plant. It will be observed that if the lateral leaflets were extended they would conceal the flower from insects flying at a higher level than the mouth of the spathe. It is therefore an advantage to the plant that they should assume this abnormal position.

I may add that the resemblance of this *Arum* to the cobra snake is very close, and cannot easily be accounted for. The diamond-shaped markings of the cobra's head are counterfeited on the spathe, as also are the lines on the neck; while the tongue-like prolongation of the spadix and of the mid-rib of the spathe serve to complete the resemblance of the plant to a living animal. As the cobra is almost unknown in the localities where this *Arum* grows, it seems that the strange mimicry can be nothing more than accidental coincidence, even if any theory of advantage to the plant therefrom could be devised. But the "counterfeit presentment" is so striking that I am convinced any person who unexpectedly saw this plant "rearing its horrid head" above the rank herbage of an Indian jungle would start back with horror.

HENRY COLLETT

Nagkunda, near Simla, June 15

Rattle-snakes in Wet Weather

I HAVE had much pleasure in reading Mr. Frank Buckland's edition of "White's Selborne." Among the notes on page 448, Mr. Buckland says:—"I know that rattle-snakes cannot play up their rattles in wet weather. The horn of the rattle becomes more or less saturated with water, and no sound can then be produced from it. By placing a rattle in a glass of water, and letting it soak a while, I find this is the case."

Mr. Buckland's dried rattle has led him into an error. The live rattle-snake can "play up" his rattle in the very wettest of wet weather. I have taken them alive on two occasions in the midst of a heavy rain, and I could discover no difference in their rattling powers. It is true, however, that rattle-snakes are seldom found in low moist places; they frequent, by preference, high and dry ground.

During the year 1873 I kept in my room a rattle-snake for eight months. In this time I came to know that "Rattler," so I called him, could "play up" several, different notes indicative of anger, of pleasure, and of loneliness.

I think that it will be found, upon proper examination, that the fangs of the rattle-snake are shed just as the teeth of other animals.

HUNTER NICHOLSON

East Tenn. University, Knoxville, Tenn., U.S.A.

Meteors

AT 9.48 last evening I saw a bright meteor pass downwards towards a Aquarius, where it disappeared. It emitted a bluish light, and although the moon was up, it shone for a few seconds with the brilliancy of Venus. A second smaller meteor passed upwards towards the zenith about 10.5. In both cases the vanishing point was near Delphinus. W. AINSLIE HOLLIS

Brighton, July 30

OUR ASTRONOMICAL COLUMN

THE HERSCHELIAN COMPANION OF ALDEBARAN.—In a communication lately received from M. Camille Flammarion, it is endeavoured to show that the change of relative situation of the small star with respect to Aldebaran, is not accounted for by the proper motion of the latter, as was stated by Struve ("Positiones Mediæ," p. ccxxvi.), but that it is necessary to admit the existence of a very appreciable proper motion of the companion, which would be the first instance of the kind in so small a star. M. Flammarion collects the various published measures and adds to them measures made by Mr. Gled-

hill in 1876, and by himself in 1877. Sir W. Herschel's measures may be set aside at once as not sufficiently exact for a discussion of a moderate proper motion; his angle in 1781 is the result of a single measure, and his distances, as is well known, are in defect, when they exceed a minute. Taking then Struve's epoch, 1836.06, as the earliest reliable datum we possess, and bringing it up to Dembowski's epoch, 1863.37, with Leverrier's proper motion for Aldebaran, we find no greater difference than may be accounted for by unavoidable error of observation, so that Struve's inference on comparing the Pulkowa measures, in 1851 with the Dorpat measures of 1836, "itaque comes motus non est particeps, sed in coelo quiesca videtur," and Dembowski's conclusion when speaking of this object and of λ Aurigæ, "Les différences s'accordent assez bien avec les mouvements propres des deux principales," are thus supported. The angle, however, should now be less than is assigned by Mr. Gledhill and M. Flammarion from their own measures ($35^{\circ}5$), and further careful measures will be desirable to clear up a possible question of personal equation. If we reduce Struve's angle for 1836 with Leverrier's proper motion of Aldebaran to the present year, we find an angle nearer $32^{\circ}5$ than $35^{\circ}5$.

THE THIRD COMET OF 1759.—This comet was not observed until January, 1760, but appears in our catalogues as comet 1759 III., from the circumstance of the perihelion passage having occurred in December of that year. It approached very near the earth, but was not a conspicuous object more than a few days. There are several references to the comet in the *Annual Register* for 1760, where we learn that it was "discovered and astronomically observed by Mr. Dunn at his Academy at Chelsea," who had determined the positions of Halley's comet on every evening during the first week of May previous. Pingré states that the sky, having been constantly overcast at Paris for several days, all the astronomers of that capital, including Cassini de Thury, Maraldi, Lacaille, and Messier, detected the comet on the evening of January 8; Dunn is credited with having found it on January 1. It was seen at Lisbon on January 7. For the purpose of these remarks we shall adopt the elements of Lacaille, in deducing the apparent places of the comet.

There is no reason why the comet should not have been found on January 1, if atmospheric conditions had been favourable, but it must have been on the morning, not on the evening of that day. In fact, the comet would rise soon after 1 o'clock A.M., in London, and would be upon the meridian a few minutes before six at an altitude of more than 23° . It is, however, distinctly stated in the *Annual Register* that Dunn discovered the comet in the evening, that "it appeared to the naked eye like Jupiter or Venus through a thick fog, and made a near appulse to the star in Orion's right knee, and moved more than four degrees of the heavens in four hours of time." This can only refer to the evening of January 8. The elements give the following positions:—

| | d. h. | R.A. | N.P.D. | Distance from earth. |
|-----------|----------|-----------|-----------|----------------------|
| January 8 | 8 ... | 88 59 ... | 99 55 ... | 0.0734 |
| " | 8 12 ... | 84 17 ... | 98 34 ... | 0.0760 |

There was, therefore a motion of upwards of four degrees in as many hours, and soon after midnight the comet would not be more than 1° from Orion's right knee, or κ Orionis. It is therefore pretty certain that Dunn did not precede other observers by a week, as might at first sight appear from the statement in the *Register*. Clouded skies had evidently prevailed in Western Europe for some days, and the comet was detected on the same evening, January 8, on the heavens clearing, in England, France, and Holland.

The rapid course of this retrograde comet will be apparent from the following positions, calculated for Greenwich midnight,

| | R.A. | N.P.D. | Distance. |
|--------|------------|------------|-----------|
| Jan. 5 | 165 36 ... | 109 22 ... | 0.112 |
| " 6 | 147 11 ... | 110 3 ... | 0.083 |
| " 17 | 116 27 ... | 106 58 ... | 0.068 |
| " 8 | 84 17 ... | 98 34 ... | 0.076 |
| " 9 | 64 9 ... | 91 30 ... | 0.101 |
| " 13 | 39 22 ... | 82 33 ... | 0.248 |

Between January 7 and 8 the comet passed over 32° in arc of great circle, and was nearest to the earth soon after midnight on the former date. On January 9 in the evening Dunn says the comet passed near μ and ν in Eridanus, and we find from Lacaille's elements that at 5h. 39m. P.M. it would have the same right ascension as μ , with only $40'$ greater declination. So far as regards position the comet might have been observed as early as the day of perihelion passage, December 16, when it was in R.A. 199° and N.P.D. 103° , rising in London at 2h. 45m. A.M.; but the intensity of light was only $\frac{1}{120}$ th of that on the evening of January 8, when it was generally discovered. It is rather unfortunate that it was not observed over a longer period, since it appears certain that in its approach to perihelion it must have passed very near to the planet Jupiter, and we might expect a sensible deviation from the parabola. On November 7, 1758, Lacaille's orbit would give the comet's distance from Jupiter less than 0.05 .

METEOROLOGICAL NOTES

SUN-SPOTS AND RAINFALL OF CALCUTTA.—Mr. E. Douglas Archibald, of Naini Tal, has written an interesting letter to *The Englishman*, the leading Calcutta newspaper, in which he shows from the observations made from 1837 to 1876, that the winter rainfall (Nov. to April inclusive) of Calcutta is marked by a distinct periodicity, the maximum rainfall occurring during the years of minimum sunspots, and the minimum rainfall during the years of maximum sun-spots. The following are the figures for the years of the sun-spot cycle beginning with the year of minimum sun-spots:—

| | Average Rainfall. |
|---------------------------|-------------------|
| 1st and 2nd year of cycle | 6.44 inches |
| 3rd and 4th | 5.93 " |
| 5th and 6th | 4.44 " |
| 7th and 8th | 5.03 " |
| 9th and 10th | 6.15 " |
| 11th | 8.49 " |

the average rainfall for the forty years being 5.41 inches. Mr. Archibald is of opinion that this peculiarity, which is the reverse of what obtains as regards the rainfall of the whole year, in its relation to sunspots, will be found not to occur much farther south than Calcutta, and that it will be more decidedly marked over the region farther to the north lying more immediately under the great range of the Himalayas. The point is one of very considerable interest and deserves the fullest investigation, since, if the supposition proves to be correct, it will doubtless lead to a more exact method of examining the rainfall in its relation to sun-spots. It may be remarked that the winter rainfall at Sydney (in the southern hemisphere) from 1840 to 1876, which is situated within the latitudes indicated by Mr. Archibald, exhibits the same peculiarity as that of Calcutta in its relation to the sun-spot period.

WINDS OF THE SOUTH ATLANTIC.—M. Brault announces the publication by the French Marine of a series of new meteorological charts giving the direction and force of the winds of the South Atlantic for each of the four seasons, the charts being similar to those published by M. Brault about two years ago on the winds of the North Atlantic. The new charts contain the results of 189,573 observations of the wind. The general movement of the winds in summer over this portion of the globe resembles

an immense whirl whose centre is about 30° - 35° lat. S., and 10° - 20° long. W. The whirling movement is in a direction contrary to that of the hands of a watch, being thus opposite to the general circulation of the atmosphere over the North Atlantic in summer. Out from this centre winds blow in all directions, the more important being the south-east trades, which are deflected to south and south-south-west off the coast of Africa, and to east-south-east and east on approaching America; then in succession north-east, north, and north-west winds on advancing southward along the coast of America, merging finally in the westerly winds which blow across the Atlantic from Cape Horn to the Cape of Good Hope. Looking both at the direction and force of the winds, M. Brault concludes that the results establish beyond a doubt the fact that, contrary to views entertained up to a comparatively recent date, there does not exist any tropical zone stretching across the South Atlantic, characterised by the prevalence of calms and light variable breezes. These results are in entire accordance with recent researches into the atmospheric movements over this region, and are of peculiar interest when viewed in connection with the distribution of atmospheric pressure and its variation with season over South America, the South Atlantic, and South Africa.

CLIMATE OF KOSSEIR, ON THE RED SEA.—In the last number of the *Journal* of the Austrian Meteorological Society, p. 225, there is an interesting article on the climate of Kosseir, on the Red Sea, based on a year's observations by Dr. Klunzinger during 1872-73. The interest of the climate of this region lies in its extreme character in certain directions, and the regularity of occurrence of its changes from season to season. The mean atmospheric pressure is $30\cdot020$ inches, rising to the maximum $30\cdot213$ inches in January, and falling to the minimum $29\cdot863$ inches in July, showing thus a variation of $0\cdot350$ inches in the monthly means. The mean temperature is $76^{\circ}\cdot3$, the warmest month, August, being $84^{\circ}\cdot9$, and the coldest, January, $64^{\circ}\cdot9$. There is little cloud in any season, and in summer the skies are constantly all but cloudless. A prominent feature of the climate is its dryness, the mean relative humidity for the year being only 56, falling in June to 51, and rising to 62 in November. This great dryness is due to the winds, which are northerly and north-westerly during the whole year, the only change being from about north-north-west in summer to north-west in winter. Occasionally, however, when easterly winds set in, the air becomes so saturated that everything is wetted with the vapour with which it is heavily charged. On June 4, 1873, a "Samum" commenced (north-north-west, force 7), the horizon having a grey troubled appearance, the sky cloudless, and the air hot and dry; it continued till the 6th, and it was during this strong dry wind that the highest temperature, $93^{\circ}\cdot9$, was observed.

DROUGHT IN CANADA.—An unusual drought has prevailed in Canada during the past spring. Little rain having fallen for ten weeks, the waters of the Ottawa and St. Maurice, two of the principal lumbering rivers, have been reduced to their summer level, having never before been so low at this season. A serious consequence of this state of matters is, that very large quantities of the finest timber of the dominion must remain in the woods till next year.

EARLY ALLUSIONS TO THE MAGNETIC NEEDLE

AT recent meetings of the Literary and Philosophical Society of Manchester interesting contributions to the subject were made. In a paper by Mr. H. Grimshaw he refers to such an allusion in a work entitled, "An Apologie of the Power and Providence of God in the

Government of the World; or, An Examination and Censure of the Common Error touching Nature's Perpetual and Universal Decay: Divided into Four Books." The author is one "G. H.," D.D. (Doctor of Divinity), and the work is printed at Oxford by John Litchfield and William Turner, "Printers to the famous University," Anno Domini 1627, being therefore exactly 250 years old.

The third book of the four into which the work is divided treats of "The pretended decay of mankind in regard and duration, of strength and stature, of arts and wits." The tenth chapter of this third book is said to be "Touching diverse artificiall workes and usefull inventions, at leastwise matchable with those of the ancients, namely and chiefly the invention of Printing, Gunnes, and the Sea-Card or Mariners Compasse." This tenth chapter again, for such is the orderly division of the subjects, is subdivided into four sections, and the fourth of these is headed "Of the use and invention of the Mariners Compasse or sea-card, as also of another excellent invention sayd to be lately found out upon the Load-stone, together with the conclusion of this comparison touching Arts and Wits, with a saying of Bodius, and another very notable one of Lactantius."

It is in the account of this "excellent invention sayd to be lately found out upon the loadstone" that a curious prevision or dream, so to speak, of the application of electricity as a means of communication occurs, and there is small wonder that the old philosopher called it as he does further on, "an excellent and secret conclusion upon the stone," for, whilst perusing his description, one can hardly imagine that the writer has not in his mind's eye one of our most modern telegraphic instruments. The paragraph is as follows:—

"Another excellent and secret conclusion upon this stone, pretended to be found out in these latter times, is, that by touching two needles with the same stone, they being severally set so as they may turne upon two round tables, having on their borders, the *Alphabet* within circlewise, if two friends agreeing upon the time, the one in Paris, the other in London (having each of them their table thus equally fitted) be disposed upon certayne dayes and at certaine houres to conferre, it is to bee done by turning the needle in one of the tables to the *Alphabet*, and the other, by *Sympathie* will turn itself in the same manner in the other table though never so farre distant: which conclusion if infallibly true, may likewise proove of good and great consequence; howsoever, I will set it down as I find it described by *Famianus Strada* in imitation of the stile and vaine of *Lucretius*."

Magnesi genus est lapidis mirabile, &c., &c.

Then follows the extract in Latin, with the English translation in verse attached.

It will be acknowledged by any one familiar with the instrument, that the dial telegraph of Cooke and Wheatstone, invented subsequently to their first upright needle form, most curiously carries out the ideal description of this old author, and it will be seen that the date at which his work is written was nearly 200 years prior to the first attempt made to communicate at a distance by means of magnetic needles.

Prof. Stanley Jevons, in a subsequent paper, stated that ten years ago he spent some trouble in investigating this curious anticipation of the telegraph, but only published the results in the form of a brief anonymous article in a weekly newspaper. This curious subject, Mr. Jevons thinks, has not received the attention which it seems to deserve, but it was not wholly unknown. The Abbé Moigno, in his "*Traité de Télégraphie Electrique*" (Paris, 1852), alludes to what he calls this "Charmant rêve, ou operation necromancienne," and he points out that Addison had quoted the remarkable verses of Famianus Strada in the *Spectator*, No. 241. Addison speaks of "a chimerical correspondence between two

friends by the help of a loadstone." Strada's remarkable lines are also quoted and translated in Mr. George Dodd's account of "Railways, Steamers, and Telegraphs: a Glance at their Recent Progress and Present State" (Chambers, 1867). Mr. Jevons found allusions to a magnetic telegraph running through many scientific, or quasi-scientific, works of the sixteenth and seventeenth centuries. Sir Thomas Browne, in his "Pseudodoxia Epidemica," says:—"The conceit is excellent, and, if the effect would follow, somewhat divine;" and he speaks of it as a conceit "whispered thorow the world with some attention, credulous and vulgar auditors readily believing it, and more judicious and distinctive heads not altogether rejecting it." Sir Thomas, it would seem, submitted the matter to experiment, but found that though the needles were separated but half a span, when one was moved "the other would stand like Hercules' pillars."

Joseph Glanvill, in his *Sceptis Scientifica* (1665), discusses the objections of Sir Thomas Browne, and concludes that "there are some hints in natural operation that give us probability that it is feasible." How can we read without wonder these words written by Glanvill more than 200 years ago? "Though this pretty contrivance possibly may not yet answer the expectation of inquisitive experiment, yet 'tis no despicable item that by some other such way of magnetick efficiency it may hereafter with success be attempted, when magical history shall be enlarged by riper inspections; and 'tis not unlikely but that present discoveries might be improved to the performance." It is evident that Glanvill treats the matter quite seriously as a scientific possibility. The Marquis of Worcester probably refers to the magnetic telegraph when he speaks of "intelligence at a distance communicative, and not limited to distance, nor by it the time prolonged." (Dirck's "Life of Worcester," p. 357.)

Mr. Jevons tried to trace these notions to the first inventor, but, as might be expected, without much success. Strada attributed the invention to the celebrated Cardinal Bembo, the secretary of Leo X., but as Bembo (who died in 1547) was a historian and literary character, it is hardly likely that he would originate a scientific conception of the sort. The earliest books in which Mr. Jevons found allusions to a magnetic telegraph is the *Natural Magic* of Baptista Porta, published in 1589. In the seventh book he describes the "wonders of the magnet," saying in the preface, "I do not fear that with a long absent friend, even though he be confined by prison walls, we can communicate what we wish by means of two compass needles circumscribed with an alphabet." In the eighteenth chapter of the same book, he describes the experiment of putting a magnet under a table and moving thereby a needle above the table. This experiment, as Porta remarks, was known to St. Augustine, and an exact description will be found in his "De Civitate Dei," a work believed to have been begun A.D. 413. It seems probable that this passage in St. Augustine suggested the notion either to Porta, Bembo, or some early Italian writer, and that thus it came to be, as Sir Thomas Browne says, "whispered thorow the world."

Mr. William E. A. Axon refers to the passage in Strada in which he supposes the loadstone to have such virtue that "if two needles be touched with it, and then balanced on separate pivots, and the one be turned in a particular direction, the other will sympathetically move parallel to it. He then directs each of these needles to be poised and mounted on a dial having the letters of the alphabet arranged round it. Accordingly, if one person has one of the dials and another the other, by a little pre-arrangement as to details, a correspondence can be maintained between them at any distance by simply pointing the needles to the letters of the required words." The date of the first edition of Hakewill's "Apologie or Declaration of the Power and Providence of God in the Government of the World" is 1627; but the work of Strada's from

which he quotes was published ten years earlier. Famianus Strada was born at Rome in 1572, and his "Prolusiones Academicæ et Paradigmata Eloquentiæ" appeared at Rome in 1617. Several editions of his "Prolusiones" have been printed in this country. The particular poem referring to the loadstone has been translated into English, and is printed in "The Student; or, Oxford and Cambridge Miscellany," 1750. The passage is referred to by Addison in a paper in the *Spectator*, No. 241, and in the *Guardian*, No. 119. In the former of these he adds: "In the meanwhile, if ever this invention should be revived or put in practice, I would propose that upon the lover's dial-plate there should be written not only the four-and-twenty letters, but several entire words which have always a place in passionate epistles: as flames; darts; die; language; absence; Cupid; heart; eyes; hang; drown; and the like. This would very much abridge the lover's pains in this way of writing a letter, as it would enable him to express the most useful and significant words with a single touch of the needle."

The subject is an interesting one, and seems to us well worth being followed out.

EVOLUTION OF NERVES AND NERVO-SYSTEMS¹

II.

AS the Medusæ are thus the lowest animals in which a nervous system has yet been discovered, we have in them the animals upon which we may experiment with the best hope of being able to elucidate all questions concerning the origin and endowments of primitive nervous tissues. I have therefore spent much time and labour, both last year and this year, in cultivating this field of inquiry; and as it is a field whose ground had never before been broken, and whose fertility has proved itself prodigious, it is not surprising that I should have reaped a rich harvest of results. So far as these results have any bearing on the general theory of evolution, their character is uniformly such as that theory would lead us to expect. For if I had two hours at my disposal instead of one, I might mention a number of facts which tend to show, in a very striking manner, that the primitive nervo-muscular tissues of the Medusæ, in respect of their physiological properties, present unmistakable affinities, on the one hand with the excitable tissues of certain plants, and on the other hand with the nervo-muscular tissues of higher animals. But not having time to go into this matter, I shall on the present occasion restrict myself to describing such of my results as tend to substantiate Mr. Herbert Spencer's theory concerning the mode in which nerves and nervo-systems have been evolved. And I adopt this course, not only because I feel that any facts bearing on so important a subject cannot fail to be of interest to all intelligent persons, but also because I think that this is a place best suited for publishing the somewhat speculative inferences which I have drawn from my facts. If these inferences are correct, their philosophical as well as their scientific influence will be great and far-reaching; but until they shall have been more completely verified I have not thought it desirable to adduce them in my communications to the Royal Society. Referring, therefore, those among you who may be interested in the research as a whole to the *Philosophical Transactions*, I will now invite your attention to a connected interpretation of some of the facts that it has yielded—an interpretation which I here publish for the first time.

To begin, then, with this diagram, Fig. 2. It represents *Aurelia aurita*, with its polypite cut off at the base, and the under, or concave, surface of the bell exposed to view. The bell, when fully expanded as here represented,

¹ Abstract of a Lecture delivered at the Royal Institution on Friday evening, May 25, 1877. By George J. Romanes, M.A., F.L.S., &c. Continued from p. 233.

is about the size of a soup-plate, and in it all the ganglia of the margin are collected into the eight marginal bodies; so that on cutting out these eight marginal bodies total paralysis of the bell ensues. But although the bell is thus paralysed as to its *spontaneous* movements, it continues responsive to stimulation; for every time you prick or electrify any part of the contractile sheet, a wave of contraction starts from the point which you stimulate, and spreads from that point in all directions as from a centre. Such contractile waves, at ordinary temperatures, travel at about the rate of a foot and a half per second; and the important question with regard to them which we shall have to consider to-night is this—Are they merely of the nature of muscle-waves, such as we see in undifferentiated protoplasm, or do they require the presence of rudimentary nerve-fibres to convey them—the *stimulus* wave in the rudimentary nerve-fibres progressively causing, as it advances, the *contractile* wave in the rudimentary muscle-fibres?

Now the great argument in favour of these contractile waves being muscle-waves, and nothing more, is simply this—that the contractile tissue is able to endure immensely severe forms of section without the contractile waves in it becoming blocked. For instance, when the bell of *Aurelia* is cut as here represented, Fig. 3, and any part of the circle is stimulated, a contractile wave radiates

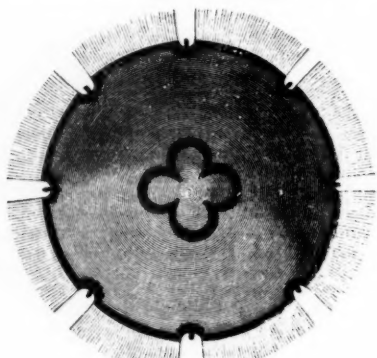


FIG. 2.

from the point of stimulation just as it did before the cuts were introduced, notwithstanding the wave has now to zig-zag round and round the ends of the overlapping cuts. Similarly, if instead of employing artificial stimulation, a single ganglion (*g*) be left *in situ* while all the other seven are removed, contractile waves will radiate in rhythmical succession from the single remaining ganglion, and course all the way round the disc. Now this experiment seems to prove that the contractile waves depend for their passage, not on the conductile function of any primitive nervous net-work, but on the protoplasmic qualities of the primitive muscular tissue. The experiment seems to prove this, because so severe a form of section would seem of necessity to destroy the functional continuity of anything resembling such a nervous net-work as we observe in higher animals.

Here, again, Fig. 4, is another form of section. Seven marginal bodies having been removed as before, the eighth one was made the point of origin of a circumferential section, which was then carried round and round the disc in the form of a continuous spiral—the result, of course, being this long riband-shaped strip of tissue with the ganglion (*g*) at one end, and the remainder of the swimming-bell at the other. Well, as before, the contractile waves always originated at the ganglion; but now they had to course all the way along the strip until they arrived at its

other extremity, and as each wave arrived at that extremity it delivered its influence into the remainder of the swimming-bell, which thereupon contracted. Hence, from this mode of section as from the last one, the deduction certainly appears to be that the passage of the contractile waves cannot be dependent on the presence of a nervous plexus; for nothing could well be imagined as

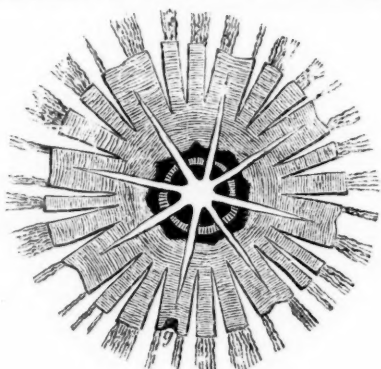


FIG. 3.

more destructive of the continuity of such a plexus than this spiral mode of section must be.

Nevertheless there is an important body of evidence to be adduced on the other side; but as I can only wait to state a few of the chief points, I shall confine my observations to the spiral mode of section. First of all, then, I have invariably found it to be the case that if this mode of section be carried on sufficiently far, a point is sooner or later sure to come at which the contractile waves cease to pass forward: they become blocked at that point. Moreover, the point at which such blocking of the waves takes place is extremely variable in different individuals of the same species. Sometimes the waves will become

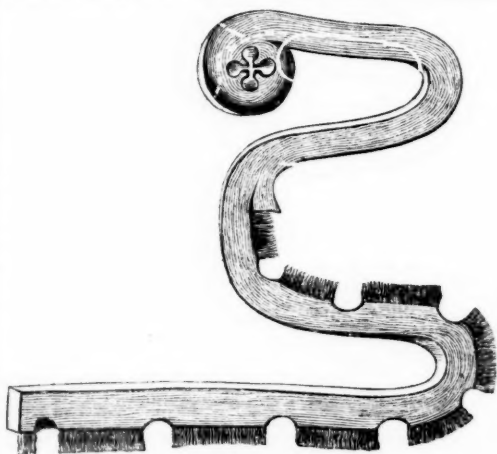


FIG. 4.

blocked when the strip is only an inch or less in length, while at other times they continue to pass freely from end to end of a strip that is only an inch broad and nearly a yard long; and between these two extremes there are all degrees of variation. Now if we suppose that the influence of the ganglion at the end of the strip is propagated as a mere muscle-wave along the strip, I cannot see

why such a wave should ever become blocked at all, still less that the point at which it does become blocked should be so variable in different individuals of the same species. On the other hand, if we suppose the propagation of the ganglionic influence to be more or less dependent on the presence of a more or less integrated nerve-plexus, we encounter no difficulty; for on the general theory of evolution it is to be expected that if such fibres are present in such lowly animals they should not be constant as to position.

But there is a still stronger argument in favour of nerve-fibres, and it is this. At whatever point in a spiral strip which is being progressively elongated by section the blocking of the contractile wave takes place, such blocking is sure to take place completely and exclusively at that point. Now I cannot explain this invariable fact in any other way than by supposing that at that point the section has encountered a line of functionally differentiated tissue—has severed an incipient nerve.

Such, some of you may remember, was the state of the evidence when I last addressed you upon this subject. On the whole I provisionally adopted the view that all parts of the rudimentary muscular sheet of the Medusæ are pervaded by a plexus of rudimentary nerves, or "lines of discharge;" and I explained the fact of the tissues in some cases enduring such severe forms of section without suffering loss of their physiological continuity, by supposing that all the rudimentary nerve-fibres composing the plexus to be capable, in an extraordinarily high degree, of vicarious action. If you were to represent the hypothetical nervous plexus by a sheet of muslin, it is clear that however much you were to cut the disc of muslin with such radial or spiral sections as are represented in the diagrams, you could always trace the threads of the muslin with a needle round and round the disc without once interrupting the continuity of your tracing; for on coming to the end of a divided thread you could always double back on it and choose another thread which might be running in the required direction. And this is what I last year stated to be my opinion as to what took place in the fibres of the hypothetical nervous plexus:—whenever a stimulus wave arrives at a cut, I imagined it to double back and pass into the neighbouring lines of discharge, which I thus supposed to act vicariously for the divided line.

Such, then, when last I addressed you, was the standing of this question as to the character of these highly remarkable contractile waves. On the whole I decided in favour of a rudimentary nervous plexus, notwithstanding the improbability that such a plexus should be capable of vicarious action in all its parts to so almost unlimited a degree.¹ I am glad to say that this decision has now

¹ This antecedent improbability is not so overwhelming as it is at first sight apt to appear; for we must remember that in a peripheral nervous plexus as we meet with it in the higher animals—i.e., in the fully evolved form of such a structure—each of the constituent nerve-fibres is provided with an insulating coat for the very purpose of preventing vicarious action among these fibres and the consequent confusion among the reflex mechanisms which such vicarious action would manifestly occasion. But because insulation of peripheral nerve-fibres is thus an obvious necessity in the case of a fully evolved nervous plexus, it by no means follows that any high degree of insulation should be required in the case of an incipient nervous plexus. On the contrary, any hypothesis as to the manner in which nerve-fibres first begin to be differentiated from protoplasm must suppose that the conductile function of the incipient nervous tracts precedes any structure, such as that of nerve-coats, whereby this function is strictly confined to particular tracts. The antecedent probability being thus in favour of the view that insulating structures are a product of later evolution than are the essential nervous structures which they insulate, it would clearly be very hazardous to draw any analogy between an incipient nervous plexus such as I suppose to be present in the Medusæ, and a fully-evolved peripheral plexus of any of the higher animals. A less hazardous analogy would be furnished by the fibres which occur in the central nervous system of the higher animals; for here it may be said, both *a priori* from Mr. Spencer's theory and *a posteriori* from histological indications, that the nerve-fibres occur in various degrees of differentiation. And that vicarious action is possible to some considerable extent through a bridge of the grey matter of the cord, has been shown by the double hemi-section experiments of Brown-Séquard. Moreover, the admirable experiments of Golz would seem to indicate that vicarious action is also possible to a large extent among the ultimate elements of the brain. I may add that recent research has tended to suggest a novel interpretation of the way in which certain poisons, such as strychnia, act upon the cord; for whereas it has hitherto been supposed that

been further justified by some additional observations which are of the first importance. For since my last lecture I have noticed the fact that reflex action takes place between the marginal ganglia of the Medusæ and all the contractile tissues of the animal. Thus, for instance, if you seize the polypite with a pair of forceps, the marginal ganglia almost immediately set the swimming-bell in violent motion—thereby showing that the stimulus must have coursed up the polypite to its point of insertion in the bell, and then down the sides of the bell to the ganglia, so causing them to discharge by reflex action. Again, suppose that seven of the eight ganglia have been removed from the margin of *Aurelia*, and that any part of the contractile disc is stimulated too gently to start a contractile wave from the point immediately stimulated, a contractile wave will nevertheless shortly afterwards start from the ganglion—thus showing that a stimulus wave must have passed through the contractile sheet to the ganglion, and so caused the ganglion to discharge. Indeed in many cases the passage of this stimulus wave admits of being actually seen. For it is a peculiarity of the numberless tentacles which fringe the margin of this Medusa, that they are more excitable than is the contractile tissue of the bell. Consequently a stimulus may be applied to the contractile tissue of the bell which is not strong enough to start a contractile wave in the bell-tissue itself, and is yet strong enough to start a contractile wave in the tentacles—one tentacle after another contracting in rapid succession until the wave of stimulation has passed all the way round the disc. The latter, of course, remains quite passive until the tentacular wave, or wave of stimulation, reaches one of the ganglia (or the single remaining ganglion, if the disc has been prepared by removing seven of the ganglia), when, after an interval of half a second for the period of latency, the ganglion is sure to discharge, and so to cause a general wave of contraction.

Now these facts prove, in a singularly beautiful manner—for this optical expression of the passage of a wave of stimulation is a sight as beautiful as it is unique—these facts, I say, conclusively prove that the whole contractile sheet of the bell presents not merely the protoplasmic qualities of excitability and contractility, but also the essentially nervous quality of conducting stimuli to a distance irrespective of the passage of a contractile wave. So I conclude there can be no longer any question that we have here to deal with a tissue already so far differentiated from primitive protoplasm, that the distinguishing function of nerve has become fully established.

THE NORWEGIAN ATLANTIC EXPLORING EXPEDITION

Tromsø, July 13, 1877

THE expedition met at the beginning with several unfavourable circumstances. In the last week of May Capt. Wille went out to Husø with the *Vöringen*, in order to determine the magnetical constants of the ship. After his arrival a flaw was discovered in the shaft, so that he went back to Bergen, where there was fortunately a new shaft lying ready. A few days later the ship was again at Husø, and was swung, not without some difficulty owing to rough weather. The *Absolute* magnetical observa-

the abnormal reflex excitability which these poisons engender is due to their exerting a stimulating influence on the cord, the researches in question have fully well proved that the very reverse is true, viz. that the action of these poisons is to depress the vitality of the cord. For a number of facts go to prove that the abnormal reflex excitability is due to the impairment of some function which has been provisionally termed "resistance of the cord," a function which in health prevents the undue spread of a stimulus through the substance of the cord, and the impairment of which by the poison consequently admits of a stimulus spreading to an undue extent, so giving rise to the abnormal reflex excitability in question. As bearing on this subject, I may observe that while the action of strychnia on the Medusæ is the same as it is on the higher animals, viz. that of causing paroxysmal convulsions, it certainly seems to exercise a depressing influence on the tissues; for an extremely weak sea-water solution has the effect of blocking contractile waves in any part of a spiral strip that is submitted to its influence.—G. J. R.

tions on shore were secured by Capt. Wille. The necessary observations for compass error having been obtained, the *Vöringen* returned to Bergen, where the scientific staff was assembled. There was, however, something still wanting before we could put to sea. The accumulators used last year had got brittle, and new ones had been ordered from London in March, but they had not arrived in May, and in answer to a telegram Capt. Wille learnt that the order had been forgotten. The new accumulators kept us waiting in Bergen till June 11, when we sailed for Stavanger, and received them on the 13th, and we put to sea at once.

Outside the coast we took a series of temperatures, which showed the minimum, not at bottom, but at a certain depth below the surface. The same phenomenon has lately been observed in all latitudes near the coast. I attribute it to the action of the winter cold on the sea.

Our first working station was in lat. $66^{\circ} 8' 5''$, long. $3^{\circ} 0' E.$, which was reached on the morning of June 16. The depth here was 805 fathoms, the temperature at bottom, $29^{\circ} 7'$. We now worked in even sections, running west-north-west and east-south-east perpendicular to the coast. The third section from lat. $67^{\circ} 53'$, long. $5^{\circ} 12' E.$ to the island of Trocnew having been completed, we went northwards into the West Fiord, where a series of temperatures was taken with Negretti and Zambra's deep-sea thermometer. Last year we could not use this instrument at sea because the slightest upward movement of the ship caused the thermometer to turn over before it had had sufficient time to accommodate itself to the temperature of the sea. This year it was fitted with a new turning apparatus devised by Capt. Wille, which proved satisfactory. In the outer part of the West Fiord the temperature on the surface was $45^{\circ} 7'$; it decreased to $38^{\circ} 8'$ in sixty fathoms; and from that point it rose to $41^{\circ} 0'$ in 140 fathoms, ten fathoms above the bottom. The Casella-Miller thermometer of course registered from this depth the minimum $38^{\circ} 8'$. The phenomenon here noticed is universal all along our coasts in the summer months; I discovered it for the first time in the West Fiord two years ago. The explanation seems to be this: In winter the air is generally cooler than the sea-surface, especially at the coast; the water is chilled from above, and the cooled layers being denser, sink down, and so the winter cold descends in the water; the temperature down to a certain depth increases with the depth. When spring and summer come, the air warms up the sea surface, and the surface layers getting warmer get lighter also, and have no tendency to sink. The temperature becomes highest at the surface, and decreases to a certain depth, below which the action of the winter cold still shows itself in a temperature increasing with the depth.

After dredging and trawling in the inner part of the West Fjord, we went to Bodö, where the expedition stayed a couple of days. On the 26th we arrived at Rösh, the outermost of the Loffoden Islands; there we stayed some days, strengthening the accumulators, cleaning the ship, taking magnetical and astronomical observations, and making excursions.

We left Rösh on the 28th at noon, and commenced our work on the sections further north, sounded, dredged, and trawled outside the Loffoden Islands till the 30th, when we went into the Hadsel Fjord, and anchored at Sortland in Westeraalen. The next week was spent in working outside Westeraalen. There the greatest depth for this year was reached, 1,710 fathoms in lat. 70° , long. $6^{\circ} 15' E.$ The Casella-Miller thermometer registered at the bottom a temperature of $28^{\circ} 4'$ when corrected for instrumental error and for pressure, the lowest temperature hitherto found by our expedition. A series of temperature observations showed that the temperature at all depths decreased with the depth, and that 32° lay in about 580 fathoms. The next Sunday, July 8, found us in Tromsö.

The expedition has this summer been favoured with remarkably fine and quiet weather, which has allowed us to carry out all our operations according to our proposed plan. The number of sounding stations is already 101; last year's total was only 93. Seventeen serial temperatures have been obtained, and the dredge and trawl have been out on the bank in the *Umbellularia* region (one specimen has been caught), and in the deep *biloculina* clay, at the depth of 1,700 fathoms, animal life was rather scarce. The boundary line between the water above and below 32° at the bottom, lies between lat. 65° and the Arctic circle as far west as $5^{\circ} 30' E.$ A little north of the Arctic circle it has a curvature towards the coast, and farther north it lies only from five to ten geographical miles off the outer side of the islands of Loffoden and Westeraalen. On this northern part the edge of the bank is very steep, and the bottom falls very rapidly towards the deep part of the Arctic Ocean. Out at sea the isothermal surface of 32° lies at very different depths in different latitudes. In the channel between Faroe and Shetland, it lies in 300 fathoms, between Iceland and Norway it sinks to 400 fathoms, and between Jem Mengon and Norway we have found it in 580 fathoms. To the westward it rises, as we found last year, east of Iceland. How it behaves further north, off Spitzbergen, we expect to find next year. Near the coast, 32° always lies at a much higher level.

The *Umbellularia* region has been found extending as far down as 880 fathoms off Westeraalen, where the specimen found came up with the weights on the dredge rope. In several places off the coast we have found, besides Norwegian rocks, specimens of chalk and flint. Of deep-sea animals, some new species have been found. On the bank off Langenes (lat. 69°) we caught plenty of the same kind of fish as are caught at the bank fisheries on the "Storeggen," off the coast of Romsdal.

The expedition is now lying at Tromsö, refitting and taking in stores for further work. We intend first to work only two more sections north of Tromsö, and then call here to make all ready for the voyage to Jem Mengon. From that island the course will be westward till we reach ice-cold water, then southwards to a point midway between Jem Mengon and Iceland, and thence to Bodö, whence the expedition will return to Bergen.

Among the novelties used in our work this year I must mention a piezometer, kindly sent me by Mr. Buchanan, chemist of the *Challenger*. This instrument has registered the depth very well. A new atmometer of my own construction has been constantly in use, giving good results. Two such instruments have, under favourable circumstances (they cannot be used in rough weather), given almost identical results; the depth of sea-water evaporated in twenty-four hours is sometimes more than four millimetres. Meteorological observations have been made every hour when at sea. The chemist has got many samples of air from the sea-water, both at the surface and at the bottom. He has taken the specific gravity of the water and determined its amount of chlorine. He has also made several determinations of its amount of carbonic acid.

MR. FROUDE'S NEW DYNAMOMETER

MR. FROUDE, in solving the problem assigned to him by the Admiralty—of producing a dynamometer calculated to test the power delivered at the end of the screw-shaft by large-sized marine engines—has enabled us to utilise a new principle of great value among the "applications of science."

In the friction-brake dynamometer, as is well known, the power delivered to a revolving shaft is measured by the rate at which a definite weight is being virtually lifted, and the number of foot-pounds of work done per minute is the circumference of the drum at the effective radius at which the weight is lifted, multiplied by the weight and by the number of revolutions per minute. Simple as the arrangement is when employed on a small scale, it

involves serious difficulties when greatly magnified, owing to the great amount of heat; and it was chiefly in order to escape this difficulty that Mr. Froude sought some fresh *modus operandi*, and ultimately felt his way to the arrangement to which we desire to draw attention.

Under this arrangement, the engine, in utilising its power, will still be virtually winding up a weight; but the weight, instead of being constant, will vary with the speed of rotation, much in the same way as the resistance of the propeller itself does; and thus the work performed by the engine under trial will more closely resemble its natural work.

The reaction, instead of arising from the continuous friction of two solid surfaces, will consist of a multitude of reactions supplied by the impact of a series of fluid jets or streams, which are maintained in a condition of intensified speed by a sort of turbine revolving within a casing filled with water, both being mounted on the end of the screw-shaft in place of the screw, the turbine revolving while the casing is held stationary; the jets being alternately dashed forward from projections in the turbine against counter-projections in the interior of the casing, tending to impress forward rotation on it, and in turn dashed back from the projections in the casing against those in the turbine, tending to resist its rotation. The important point is, that the speed of the jets is intensified by the reactions to which they are thus alternately subjected; and thus in virtue of this circumstance a total resistance of very great magnitude is maintained within a casing of comparatively very limited dimensions.

The nature of this arrangement will be gathered from the accompanying figures.

In Fig. 1, AA represents the screw-end of the screw shaft; BB shows in section what has been termed "the turbine;" it is a disc or circular plate, with a central boss, keyed to the screw-shaft in place of the screw, and revolving with the shaft. The

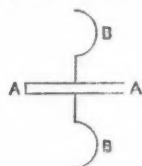


FIG. 1.

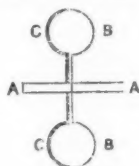


FIG. 2.

disc is not flat throughout, its entire zone being shaped into a channel of semi-oval section, which sweeps round the whole circumference concentrically to the axis. To give definiteness to the conception, imagine that, to deal with an engine of 2,000 Ind. horse-power, making 90 revolutions per minute, the diameter of the turbine-disc to the outer border of the channel is five feet.

In Fig. 2, Fig. 1 is repeated, and what has been called "the casing" is added, being indicated by the letters C C, D D, the former representing the front and the latter the back. The face carries a channel, the counterpart of that carried by the disc, which it also fronts precisely, so that the two semi-oval channels in effect form one complete oval channel, though the two halves are in substance separated by an imaginary plane of division. The back of the casing embraces or includes the disc entirely, but without touching it; the casing is also provided with a boss, which is an easy fit over that of the disc or turbine, and thus the disc carried by the shaft can revolve within the casing without touching it, while the casing itself is stationary, and one half of the oval channel is running round while the other half is at rest.

Thus far the two half channels have been regarded as open and unobstructed; they are, however, in fact each closed or cut across by a series of fixed diaphragms, a single one of which is shown in Fig. 3, as in its place in the disc-channel. The diaphragms cut the channel, not perpendicularly, but obliquely, being semicircular in outline, so that when placed obliquely their circular edges fit the oval bottom of the channel, while their diameters span the major axis of the oval. Fig. 4 shows one of the diaphragms seen end on or edgewise, as it would appear in an edgewise view of the turbine if this were transparent.

Each half channel has twelve of these diaphragms, and is thus divided into a series of cells, each of which, if viewed at right angles to one of the diaphragms, or what is the same thing, if shown in a section taken parallel to one of them, is semicircular in outline; and if thus viewed in connection with the cell which is for the moment opposite to it in the counterpart half channel, the two together make one complete cell with circular outline.

Thus the whole oval channel may be regarded as a series of obliquely placed circular cells, and as the function of the turbine is to rotate while the casing remains at rest, one half of each cell is moving past the other half in such a manner that the moving half, if viewed from its stationary counterpart, would by reason of the oblique direction of the diaphragms which form the cell sides, appear to be advancing antagonistically towards it; indeed the motion virtually constitutes such an advance, because the bottom of each moving half cell is continually growing nearer to the bottom of the stationary half cell which it faces. The effectiveness of the combination to resist rotation will be seen to depend essentially on this quasi-antagonistic virtual approach of the moving to the stationary half cell.

The channel and the whole casing is filled with water, and the turbine is made to rotate as described. When the turbine is thus put in motion, the water contained in each of its half cells is urged outwards by centrifugal force; and in obeying this impulse it forces inwards the water contained in the stationary casing half cells, and thus a continuous current is established, outward in the turbine's half cells, inward in those of the casing.

Now the action of these cells on the water contained in them may be rendered more clear by the following illustration:—Suppose a person in a railway train moving at a certain velocity to hold a racket fixedly in his hand and a ball thrown to him strikes the racket; also that there is provided a series of walls beside the railway inclined at such an angle that the ball leaving the racket and striking one of the walls will rebound to the racket. Suppose, also, for the sake of simplicity, that the ball is perfectly rigid while the walls and racket are perfectly elastic. On striking the racket for the first time the ball will rebound with a velocity equal to double that of the train added to the original velocity of projection. In order to see this clearly we must look at what

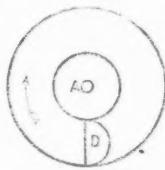


FIG. 3.

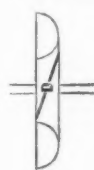


FIG. 4.

takes place in the racket. The ball meets the racket with a velocity equal to that of projection added to that of the train, the strings of the racket stretch to such an extent that their recoil would cause the ball to rebound with the sum of these velocities. At this instant suppose the train stopped, and we should then see the ball projected through the air with the sum of these velocities in consequence solely of the elastic reaction of the racket. But in the experiment we are supposing the train is not stopped but is at this instant capable of impressing on a ball at rest the full velocity of the train; this it does equally well on the ball, which we imagine, for the instant, resting against the strained racket. Thus we see that the ball will be projected through the air with a velocity equal to that of the train in addition to that with which it would have been impressed on it by the racket had the train been stopped at the moment indicated, or, in other words, its velocity of projection will, after one contact with the racket, have had added to it twice the velocity of the train. Each time the ball, rebounding from the walls in succession, meets the racket, an additional double train-velocity will be impressed on it. Another important point in this illustration must not be passed over. The action of the ball on the racket tends to retard the train, and that on the walls tends to push them forward in the direction of train as well as away from the line of railway.

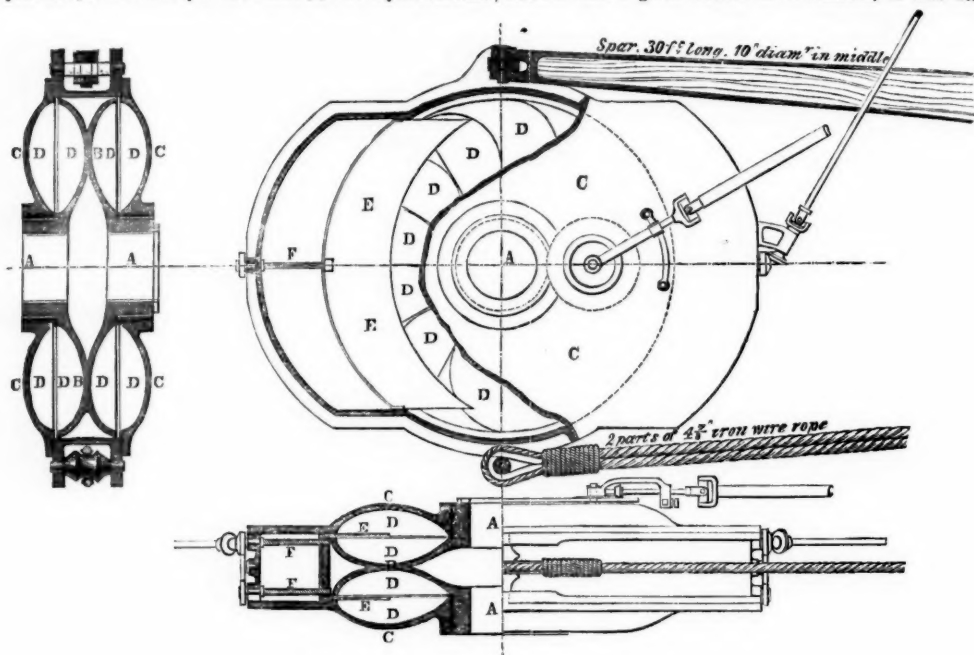
In an analogous manner the currents originated solely by centrifugal tendency, in being unceasingly reversed in direction and increased in velocity by the opposing cells, produce a resistance to the rotation of the turbine which is measured by the torsion it produces on the casing. This torsion is most conveniently measured by a spring balance attached to the end of a lever made fast to the casing.

The manner in which the currents, when established, produce the dynamometric reaction, can be traced very easily. The explanation already given of the internal form of the cells which the current traverses, shows that the volume of water which constitutes it in each complete cell may be regarded as a circular

plane or disc of water, rotating in its own plane between the diaphragms, which define the direction of the water disc and which are the boundaries of its thickness.

Having now traced the *modus operandi* by which the reaction is produced, it is necessary to show that (1) an adequate amount

of total reaction can be produced by an instrument of conveniently limited dimensions; and that (2) an instrument of given dimensions is governable as regards its reactions, that is to say, is capable of being made to produce at pleasure a greater or less reaction with a given number of revolutions, so that within

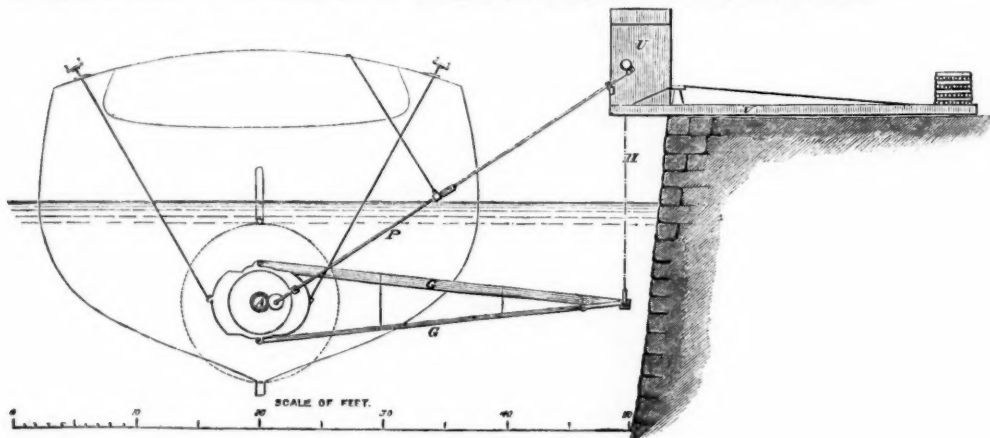


Enlarged view of dynamometer attached to screw shaft.

reasonable limits the same instrument shall be capable of dealing with engines of great or small power, allowing each to make its proper number of revolutions.

As regards condition No. 1, the theory shows, as will appear

in the appendix, that comparing two strictly similar but differently dimensioned instruments, their respective "moments of reaction" with the same speed of rotation in each, should be as the fifth powers of their respective dimensions.



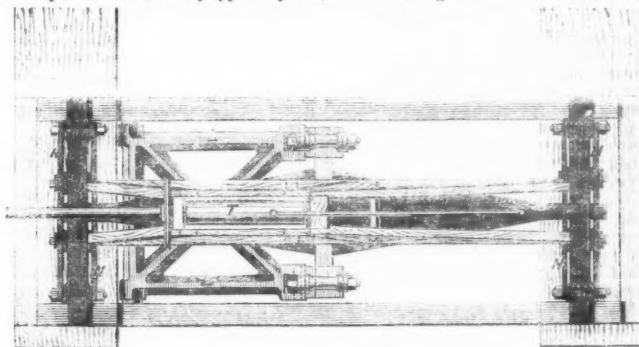
General view of dynamometer attached to screw shaft.

This proposition is fully borne out by experiment. Mr. Froude has had a pair of similar instruments made, in which the turbine diameters are respectively 12 in. and 9.1 in. Now $\left(\frac{12}{9.1}\right)^5 = 4$, and accordingly the ratio of the moments of the two instruments

at a given speed of turbine rotation should also have been 4. The ratio was in fact 3.86; but the small difference is referable to the circumstance that in the larger of the two instruments the internal surface was rather less smooth and the friction of the water consequently rather greater than in the other. The data

thus obtained not only verify the scale of comparison based on the 5th power of the dimension, but they also furnish a starting-point by which to quantify the dimensions of the instrument which will be required to deal with any given horse-power delivered with a certain number of revolutions per minute; and it thus appears that to command the measurement of 2,000 horse-power delivered with 90 revolutions per minute (a fairly typical speed

for the power), an instrument of the dimensions shown in the accompanying drawings will suffice, the turbine being 5 ft. in diameter, and being in fact a duplicate turbine, or formed with two faces, with a double-sided casing to match. This two-faced arrangement, it may be added, while it supplies a doubled circumferential reaction with a given diameter, has the advantage of obliterating all mutual thrust on the working parts; the



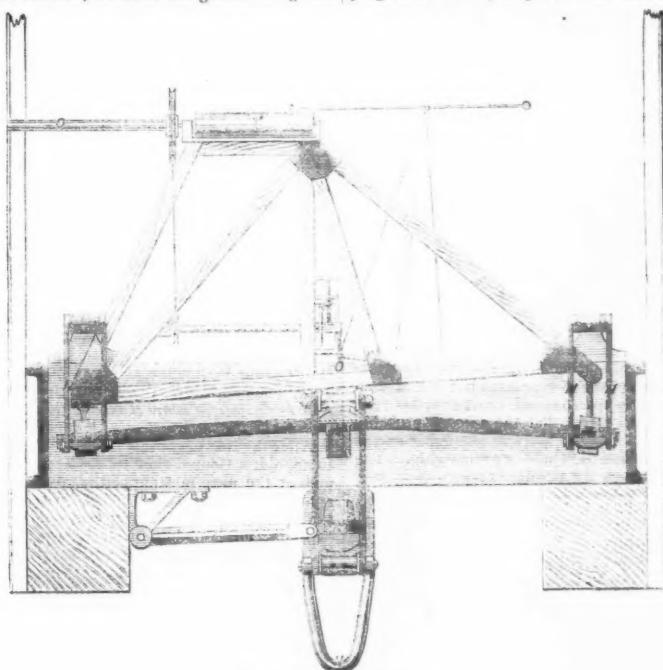
Integrating apparatus. Plans.

centrifugal forces of the double set of vortices pressing with equal intensity on the two internal opposite faces of the rigid casing.

As regards condition No. 2, the theory suggests that, by contracting the internal waterways, that is to say the passages through the cells in the turbine and the casing, and thus intercepting the free vortical rotation, all other things remaining the

same, the moment of reaction due to a given speed of rotation could be greatly reduced.

The experiments with the models fully bore out this anticipation also, and proved that, by the very simple arrangement shown in the drawings, the reaction with any given speed of turbine rotation can be reduced with a perfectly graduated progression in any required ratio down to 1-14th, the object



Integrating apparatus. Section.

being effected by advancing, from recesses in the casing abreast of the two opposite quadrants in each turbine, a lunette-shaped sliding shutter of thin metal, so fitted as to be carried forward (by a screw motion governed from the outside) along the divisional plane between the turbine cells and the casing cells. The intensity of the reaction is thus brought completely and easily under command; and in virtue of it, it follows that the instrument represented in the drawing, which, as already stated, is capable

of dealing with an engine of 2,000 horse-power, making ninety revolutions per minute, is also capable of dealing with one of only 340 horse-power, making 120 revolutions per minute. And as the reaction of the instrument varies as the square of its speed of rotation, and the horse-power delivered through it consequently varies as the cube of the speed of rotation (that is to say, with a given setting of the shutter), and as, moreover, this law of variation is somewhat the same as that which the engine itself

experiences when propelling the ship under natural conditions, it follows that the same setting of the shutters which suits a given engine when working with its highest speed and power will also approximately suit it when eased down to its lowest.

It remains to be explained in detail how it is proposed to carry out the operation in dealing with any given ship.

The ship, before she leaves the dock for the trial of her machinery, will have the instrument mounted as described, in place of her screw. The casing will be provided with proper apertures, capable of being closed at will, to permit the egress of air and the ingress of water as the dock fills. The casing will thus be in a condition to receive the moment of rotation delivered by the screw and communicate it to the recording apparatus.

The arrangement of the dynamometric apparatus presents no difficulty. In this, the downward pull delivered by the lever operates vertically on the middle of a flat horizontal steel spring, which is supported at both ends; and it is proposed so to proportion the spring that its maximum deflection shall be about $1\frac{1}{2}$ inches. Different springs, however, would be required for engines of widely different power.

Reference has previously been made to the amount of heat developed by friction in the friction brake, as probably the most formidable of the objections to its employment when the horse-power to be dealt with is as large as that now contemplated. But it must not be supposed that the absorption of the same amount of work in the instrument that has been described will fail to be converted into the same amount of heat here also. The dynamic theory of heat is unquestionable as a theory, and the quantitative relation of work and heat is known with certainty within far narrower limits than deserve even to be mentioned in reference to the present subject. Although, however, the extinction of say 2,000 horse-power will in fact here, as well as in the friction brake, consist in its conversion into so many units of heat, the circumstances of the conversion are entirely different in the two cases, and the difference is such as to obliterate here the inconvenience which was fatally great there. There, the heat was to be dealt with as being constantly developed between surfaces in close contact and inaccessible to water. Here, it will be making its appearance in the body of a mass of water; and though the rapidity of the development will be so great that the whole contents of the casing would be quickly raised to boiling heat if the heat had no escape, yet, in the first place, there is a considerable refrigerating power always at work, since the whole casing is enveloped in cold water, and, moreover, there is no difficulty in creating a constant change of water within the casing sufficient to keep down the mean internal temperature to any limit which may be thought proper. For instance, when the instrument is dealing with 2,000 horse-power, the temperature would be kept well below the boiling point if in each minute eight cubic feet of cold water be substituted for the same quantity of the hot contents of the casing, nor would the exactness of the dynamometric action be in the smallest degree impaired by the substitution.

Mr. Froude in his valuable paper, to which we are glad to call attention, thus summarises briefly the advantages which would be derived from the system of submitting marine engines to dynamometric trial. It is certain that a very large but unmeasured amount of power is wasted, in friction and otherwise, between the cylinders and the propeller; and that the amount probably differs, both in respect of difference in type of engine and in respect of goodness of construction and workmanship. The chief difficulties which thus arise are as follows:—

- (1) The speed attained by a given ship, driven by a given indicated horse-power, fails to measure discriminatively the merits of the ship.
- (2) No means exist of testing which type of engine delivers the largest proportion of the power which it indicates.
- (3) No test exists by which to measure concisely the specific constructional merit of this or that engine, or to determine the relative constructional merit of the engines supplied by different firms.

The dynamometric test would remove at once each of these difficulties, by substituting a final and real test for a collateral and to a large extent a delusive one. For to rely exclusively on the test furnished by the indicator is almost equivalent to testing the power of a horse solely by the quantity of food he consumes and digests, or the efficiency of a boiler solely by the quantity of coal per hour it will legitimately consume on its firebars.

Table of Reference Letters used in Diagrams and Drawings.

- A. Screw shaft.
- B. Turbine.
- C. Casing.
- D. Diaphragms.
- E. Sliding regulating shutters.
- F. Screws for moving E, governed by telescopic rods actuating bevel gear controlled from ship's deck.
- G. Lever for holding casing.
- H. Links connecting G with dynamometric apparatus.
- K. Knife-edged gimbal for carrying strain of H to spring.
- L. Framed radius for guiding K and eliminating oblique strains.
- M. Dynamometer spring.
- N. Suspension links carrying the ends of M.
- O. Feeler conveying elastic motion of M.
- P. Telescopic rod taking rotation of screw shaft by bevel gear and communicating it to integrating apparatus.
- Q. Motion axis of integrating apparatus governed by O.
- R. Automatic integrator.
- S. Bell crank for magnifying motion of O and conveying it to paper cylinder.
- T. Paper cylinder recording magnified motion of O.
- The graphic integration of the record given by T is comparable with the automatic integration given by R.
- U. Shed covering integrating apparatus.
- V. Strong balk brackets upholding U.

THE COMMISSION OF THE FRENCH ACADEMY AND THE PASTEUR-BASTIAN EXPERIMENTS

IN further reply to a communication of mine to the Academy of Sciences of Paris on July 10, 1876, and as his latest contribution to a controversy which grew out of it, M. Pasteur, at the *séance* of January 29, 1877, threw down a very definite challenge.

The discussion was raised according to M. Pasteur by my statement, "that a solution of boiled potash caused bacteria to appear in sterile urine at 50° C., after it had been added to the latter in quantity sufficient for exact neutralisation," and he then said:—"I defy Dr. Bastian to obtain, in the presence of competent judges, the result to which I have referred with sterile urine, on the sole condition that the solution of potash which he employs be pure, *i.e.*, made with pure water and pure potash, both free from organic matter. If Dr. Bastian wishes to use a solution of impure potash I freely authorise him to take any in the English or any other Pharmacopœia, being diluted or concentrated, on the sole condition that that solution shall be raised beforehand to 110° for twenty minutes, or to 130° for five minutes. . . . This is clear enough, it seems to me, and Dr. Bastian will understand me this time."

At the *séance* of February 12 my reply was read. The essential part of it was as follows:—"During the last week I have repeated my experiments several times, and with a degree of precaution going much beyond the severity of the conditions prescribed by M. Pasteur. . . . I repeated them at first with liquor potassæ which had been previously raised to 110° C. for sixty minutes, and afterwards with liquor potassæ which had been raised, in the same manner, to 110° C. for twenty hours. The results have been altogether similar to those produced upon sterile urine by liquor potassæ which has been raised only to 100°, when added in suitable quantity; that is to say, in twenty-four to forty-eight hours the urine was in full fermentation and swarmed with bacteria."

After the reading of this reply, M. Pasteur asked the Academy to appoint a Commission to report upon the subject in dispute, and at the next meeting of the Academy (February 19) it was announced that "MM. Dumas, Milne Edwards, Boussingault sont désignés pour constituer la Commission qui sera appelée à exprimer une opinion sur le fait qui est en discussion entre M. le Dr. Bastian et M. Pasteur."

The following correspondence then ensued:—

20, Queen Anne Street, W., February 27, 1877

DEAR SIR,—I was pleased to learn, from the *Comptes Rendus*, yesterday, that the Academy had appointed you together with MM. Milne Edwards and Boussingault to act as a Commission to "express an opinion on the fact" now under discussion between M. Pasteur and myself.

I can scarcely suppose that the Commission would deem it expedient to express an opinion on this subject without having an opportunity of seeing both M. Pasteur and myself perform our respective experiments.

I write, therefore, to inform you that if a convenient time can be arranged, I shall be very happy to come to Paris for three days

in order to perform my experiments before the Commission which has been nominated by the Academy.

I should, moreover, feel much obliged if you will have the goodness to inform me exactly what steps the Commission proposes to take, and how the precise terms for formulating the question of fact which is to be submitted to their consideration are to be settled. It appears to me that these terms ought, in the first place, to be agreed upon between M. Pasteur and myself.

Faithfully yours,

H. CHARLTON BASTIAN

Monsieur Dumas, le Secrétaire perpétuel,
Académie des Sciences

No reply to this letter was received, though a translation of it was published shortly afterwards in the *Comptes Rendus*. The first letter which subsequently came to hand on this subject was the following:—

Académie des Sciences, Paris, le 5 mai, 1877

MONSIEUR,—Je crains que la lettre que j'ai eu l'honneur de vous adresser il y a trois semaines ne vous soit pas parvenue, et je prends la liberté de vous faire savoir de nouveau que la Commission chargée par l'Académie des Sciences de prendre connaissance de vos expériences est prête à vous recevoir. Elle a déjà demandé à M. Pasteur d'opérer sous ses yeux.

Puisque vous avez accepté de venir à Paris, tout est préparé pour vous recevoir et dès votre arrivée, si vous voulez bien m'en informer, le laboratoire de l'Ecole Normale, ou tout autre, seront mis à votre disposition.

Agréez, Monsieur, l'assurance de mes sentiments les plus distingués,
J. B. DUMAS

Paris, rue St. Dominique, 69

20, Queen Anne Street, W., London, May 8, 1877

DEAR SIR,—On February 27 I had the honour of informing you that I was willing to come to Paris to perform some experiments before the Commission appointed by the Academy, if a convenient time could be arranged. I asked also to be informed as to the steps the Commission proposed to take, and how the precise question submitted to them for report was to be agreed upon.

I anxiously awaited a reply to this letter for some time, but none came.

This morning I had the honour of receiving a letter from you, bearing the date of May 5, which was reposted to me from a wrong address, viz., 81, Avenue Road, Regent's Park. Therein you state that you had written to me three weeks previously. I shall be glad if you will be good enough to inform me to what address this first letter was sent, as it has not yet come to hand; and I find, on inquiry, that it has not been delivered at 81, Avenue Road, where I resided two years ago. On receipt of this information I will make further inquiries at the General Post Office.

The letters which I have had the honour of addressing to you concerning my communications to the Academy have always borne the address which stands at the head of this sheet.

Three weeks ago, if the arrangements made by the Commission had been satisfactory, I could have gone to Paris without much inconvenience; now, however, my engagements, both public and private, will not permit me to leave London, and I fear it may be impossible for me to go to Paris till about the third week in July, when our medical session will terminate.

Meanwhile I trust to be able to recover your first letter, and I hope to be fully informed, not only as to the precise question on which the Commission is to report, but as to the mode in which the Commission will conduct the inquiry. I am still anxious, in fact, to receive that information for which I asked in my letter of February 27.

Believe me, dear Sir, faithfully yours,

Monsieur Dumas

H. CHARLTON BASTIAN

18 mai, 1877, Paris

MONSIEUR,—Je me suis empressé de vous faire un duplicata de la lettre que j'avais eu l'honneur de vous adresser au nom de la Commission de l'Académie des Sciences, dès qu'elle avait été délivrée des soins de la séance publique tenue le 23 avril, et qui ne vous était pas parvenue.

J'ai vu M. Pasteur. Il se tient à votre disposition pour le 15 juillet, époque à laquelle vous seriez libre de venir à Paris.

M. Lockyer, qui a passé quelques jours ici, s'est chargé de vous dire combien nous désirons voir avec vous vos expériences et avec quelle entière liberté d'esprit elles seront appréciées.

Agréez, Monsieur, l'assurance de ma considération la plus distinguée,
J. B. DUMAS

rue St. Dominique, 69

Académie des Sciences, Paris, le 25 avril, 1877

Duplicata.—Le Secrétaire perpétuel de l'Académie à Monsieur le Docteur Charlton Bastian, 20, Queen Anne Street, à Londres

MONSIEUR,—La Commission nommée par l'Académie des Sciences pour examiner le dissentiment qui s'est élevé entre M. Pasteur et vous a consacré plusieurs séances à suivre les expériences de M. Pasteur. Elle est donc en mesure de s'occuper des vôtres.

Puisque vous avez offert de venir les répéter devant elle à Paris, elle se met à votre disposition, et elle vous offre le laboratoire qu'il vous plaira de désigner pour les accomplir. Vous choisirez vous-même, après les avoir visités, celui qui vous conviendra le mieux. M. Pasteur vous prie de considérer le sien comme tout à vos ordres.

La Commission, avant d'engager tout examen de la question, a pensé qu'il convenait d'abord de voir les expériences mêmes, réalisées en liberté par leurs auteurs. S'il y a lieu d'ouvrir plus tard entre elles une comparaison contradictoire, elle en déterminera les conditions, en vue de donner, à son opinion, une base certaine.

Le premier élément de l'enquête à laquelle vous avez souscrit, M. Pasteur et vous, devait consister, en effet, à donner à chacun de vous l'occasion de produire les faits sur lesquels vos opinions respectives se fondent.

Agréez, Monsieur, l'assurance de mes sentiments les plus distingués.
J. B. DUMAS

20, Queen Anne Street, W., May 24, 1877

DEAR SIR,—I have the honour to acknowledge the receipt of the duplicate of the missing letter, bearing date April 25, and also your note of May 18, the assurances in which were very gratifying to me.

Your official letter of April 25 contains some information in regard to the conduct of the inquiry by the Commission, which I have been for some time desirous of obtaining. In respect to these proposed proceedings I may perhaps now be permitted to make some observations, in order, as far as possible, to avoid the chance of any misunderstanding between M. Pasteur and myself and the Commission, during the progress of the inquiry.

I am anxious, in fact, to define (1) what I understand to be the object of the Commission, and (2) to explain to what extent I am prepared to submit to its judgment. I desire to do this in order that I may have the honour of learning from you whether I am correct in this understanding, and whether my submission to the extent to be specified is all that the Commission will expect from me.

1. I gather from the *Comptes Rendus* of February 19, that the Commission has been appointed that it may "express an opinion upon the fact" under discussion between M. Pasteur and myself; and the fact in question seems to me to be this:—*Whether previously boiled urine, protected from contamination, can or cannot be made to ferment and swarm with certain organisms by the addition of some quantity of liquor potasse which has been heated to 110° C., for twenty minutes at least.* M. Pasteur asserts that he has not seen fermentation occur under these conditions, whilst I assert that I have; so that the point of principal importance would seem to be to ascertain whether such positive results can be reproduced before the Commission. I learn, therefore, with much satisfaction, that the Commission will allow to each of us the opportunity of reproducing before it the facts upon which we found our respective opinions. This, indeed, I regard as an essential condition of the inquiry.

2. If the Commission proposes to limit itself to reporting upon this mere question of fact I will freely submit to its decision. If, however, it does not propose thus to restrict itself, and is empowered to express an opinion upon the interpretation of the fact attested, and on its bearings upon the "Germ Theory of Fermentation," or "Spontaneous Generation," then I must respectfully decline to take part in this wider inquiry.

I feel compelled to adopt this decided position because my stay in Paris must be limited to three or four days; and if any other questions beyond that above specified were subsequently raised by the Commission demanding the performance of some new experiments, either by M. Pasteur or myself or by both of us, the inquiry, instead of being limited to a few days, might be prolonged indefinitely.

I desire, therefore, to obtain the assurance of the Commission that no new experiments shall be demanded from either of us, except with the full concurrence of both M. Pasteur and myself. Under these circumstances I will undertake, so far as it lies in my power, to be in Paris by the 14th of next July, in order to place myself at the disposal of the Commission.

Believe me, dear Sir, faithfully yours,
Monsieur Dumas H. CHARLTON BASTIAN

20, Queen Anne Street, W., June 21, 1877.

DEAR SIR,—One month ago (May 24) I had the honour of writing to you to ask for some official information as to the precise scope of the inquiry to be made by the Commission appointed by the Academy of Sciences, before whom I have been invited to appear. To this letter I have as yet received no reply, so that I do not even know whether it has been received.

I have made arrangements which will enable me to go to Paris and perform my experiments before the Commission at the time named in your letter of May 18, namely, about July 15, but naturally before taking part in any arbitration I desire to receive some official intimation as to the exact terms and scope of the question which has been submitted to the arbitrators. I know not whether the few lines which I saw in the *Comptes Rendus* of February 19 announcing the nomination of the Commission, contain also the only instructions which have been given to it, or whether any other and fuller instructions exist. No information has been communicated to me and I am, unfortunately, not acquainted with the custom of the Academy in regard to commissions of this kind.

Craving the favour of an early reply,
Believe me, dear Sir, faithfully yours,
Monsieur Dumas H. CHARLTON BASTIAN

MONSIEUR,—Il est parfaitement entendu que la Commission de l'Académie des Sciences sera le 15 juillet à votre disposition. Il est également qu'elle desire, si c'est possible, n'avoir à s'occuper que de l'expérience de M. Pasteur et de la vôtre, au sujet de l'urine traitée par la potasse.

Vous n'avez donc aucun motif de craindre qu'elle ait besoin de vous demander un séjour prolongé.

Veuillez agréer, Monsieur, l'assurance de ma considération la plus distinguée.
J. B. DUMAS
r. St. Dominique, 69

20, Queen Anne Street, W., July 6, 1877

DEAR SIR,—I beg to acknowledge the receipt of a letter from you which came to hand on June 29.

I do not find in it any distinct acceptance of the conditions mentioned in my letter of May 24, as those upon which alone I should be prepared to repeat my experiments before the Commission, viz., (1) the limitation of the report to the question of fact mentioned, (2) the assurance that no new experiments shall be demanded from either of us except with the full concurrence of both M. Pasteur and myself.

I might infer from your silence that no objection is raised to these restrictions, but before leaving for Paris I must receive your definite assurance that this is so.

Not being thoroughly proficient in the French language I presume the Commission will permit me to avail myself of the services of some French friend as an interpreter. I also trust that the Commission will provide for the taking of shorthand notes of any discussion during the progress of the investigation of which the Commission, M. Pasteur, or myself may desire to have a record.

On the receipt of a favourable reply you may expect me to be in Paris on Saturday morning, the 14th inst., otherwise I shall be most reluctantly compelled to decline to participate in the inquiry.

Believe me, dear Sir, faithfully yours,
Monsieur Dumas H. CHARLTON BASTIAN

Paris, 12 juillet, 1877

MONSIEUR,—La Commission de l'Académie des Sciences sera dès le 15 à votre disposition.

Elle est prête à vous entendre; mais elle desire, comme vous, que son examen soit borné au point en discussion entre vous et M. Pasteur. Ce serait seulement au cas où vous desiriez aller plus loin qu'elle aurait à examiner si le temps lui permet d'entreprendre davantage, votre séjour étant très court.

M. Edwards, membre de la Commission, parle très bien l'anglais.

Dès votre arrivée vous auriez la bonté de m'en informer, rue St. Dominique, 69.

Agréez, Monsieur, l'assurance de ma considération la plus distinguée.
J. B. DUMAS

Having received this acceptance of the limitations which I had specified, I left London for Paris on July 13.

On the afternoon of July 15, I met the Commission by arrangement at the laboratory of M. Pasteur, at the Ecole Normale Supérieure. The Commission was represented by MM. Dumas and Milne Edwards, M. Boussingault having been compelled to withdraw on account of a recent domestic affliction.

The first stage of our discussion was the announcement to me by M. Milne Edwards of his objection to the second condition mentioned in my letter of July 6, and of his determination to take no part in the inquiry if I still adhered to this condition. M. Dumas' letter of July 12, in the name of the Commission, and on the faith of which I had come to Paris, was thus at once set aside.

M. Milne Edwards contended that he could not take part in any Academy Commission which had not full power to vary the experiments at discretion; whilst I, on the other hand, contended that my stay in Paris must, as I had said from the first, be limited to a few days, and that I could not see my way, therefore, to consent to the initiation of new experimental conditions. I further urged that the Commission had been appointed to report upon a simple question of fact, that M. Pasteur had challenged me to obtain certain results, before "competent judges," that I had come to Paris to repeat certain well-defined experiments before them, and that they were commissioned to express an opinion thereon and on the experiments of M. Pasteur to the Academy of Sciences.

A very long discussion ensued, but no satisfactory conclusion was arrived at. In the evening I wrote the following note to M. Dumas:—

Grand Hôtel St. James, Paris, July 15, 1877

DEAR MONSIEUR DUMAS,—After our conference this afternoon I had a long conversation with M. Pasteur, and am going to his laboratory early to-morrow morning, to show him the mode in which I make my experiments. I shall thus be enabled to learn what precise alterations he would desire in order that the experiments may be conducted in a manner satisfactory to himself.

Afterwards I trust it may be more possible for me to meet the wishes of the Commission in regard to the inquiry, and I hope you will therefore be able to make it convenient to see me for a few minutes at your own house to-morrow at 1.30 P.M.

If you are able to do this, pray do not take the trouble to answer this note. Should it not be convenient to you, perhaps you will kindly send a few words to me to the care of Professor Würtz, upon whom I am to call about noon.

Believe me, dear Sir, faithfully yours,
À Monsieur Dumas H. CHARLTON BASTIAN

At my interview with M. Dumas on Monday, July 16, I proposed a kind of compromise. The proposition was that on the present occasion we should have "the first element" of the inquiry as defined by M. Dumas in his letter of April 25; viz., that the opportunity should be given to M. Pasteur and myself of repeating (without variation) the actual experiments upon which we based our respective opinions; that I should then return to London, and after the Commission had expressed its opinion to M. Pasteur and to myself as to any variations in the experimental conditions which they might desire to institute, that I should return to Paris to witness and to perform such modified experiments.

The names of MM. Frey, Trécul, Robin, and Würtz had been mentioned as persons one or other of whom I should like to see placed on the Commission in succession to M. Boussingault. But at the meeting of the Academy that afternoon it was announced that M. van Tieghem had been nominated to succeed M. Boussingault. This gentleman being a former pupil and present colleague of M. Pasteur, the Commission was left without a single member who could be considered as representing my views, or even as holding a neutral position between me and my scientific opponent.

The next day I received the following note from M. van Tieghem:—

Paris, 17 juillet, 1877

MONSIEUR LE DOCTEUR,—La Commission de l'Académie se réunira demain, mercredi, à huit heures du matin au laboratoire de M. Pasteur à l'Ecole Normale. Je viens, en son nom, vous prier

de vouloir bien vous y trouver pour procéder à la mise en train des expériences en litige.

Veuillez agréer, Monsieur, l'expression de mes sentiments les plus distingués.

PH. VAN TIEGHEM,
Membre de la Commission

I made all the necessary arrangements that [afternoon in M. Pasteur's laboratory for the performance of my experiments, and the next morning at eight o'clock M. Pasteur and I were at the appointed place. M. van Tieghem was also there, and shortly afterwards M. Milne Edwards arrived. He apparently had had no communication with M. Dumas since the time of my interview, and when told, in reply to a question of his, of the proposition which I had made to M. Dumas, M. Milne Edwards very hastily expressed his disapproval of it, and at once, without listening further, left the laboratory. He was followed by M. van Tieghem. I remained, and after one hour M. van Tieghem returned. He informed me that, having waited in vain for the arrival of M. Dumas, M. Milne Edwards had at length gone away.

I remained in conversation with M. van Tieghem for nearly an hour in an upper room of M. Pasteur's laboratory. When we came down, much to my surprise, we learned from M. Pasteur that M. Dumas had arrived, that he had been told of the departure of M. Milne Edwards, and that he also had then left, saying that the Commission was at an end—but without in any way communicating either with his colleague, M. van Tieghem, or with myself.

Thus began and ended the proceedings of this remarkable Commission of the French Academy.

July 30

H. CHARLTON BASTIAN

NOTES

FROM correspondence which we have received, we gather, that because we omitted to state in our leading article of last week the fact that London is the only University which treats science as a necessary branch of education, that article has been thought hostile to the University of London. The fact in question is of course well known and appreciated, but it did not seem to us to be relevant. Our article had reference to the question of Universities as against Examining Boards rather than to the quality of the examinations. We heartily acknowledge the good the London Examining Board has done, and the obligations under which it has placed science and scientific men.

THE Annual Conference of the Royal Archaeological Institute of Great Britain and Ireland commences, on the 7th proximo, at Hereford, for a week. The Bishop of Hereford is president.

AN important resolution of the International Geodetic Congress is now being carried out. The Montsouris observatory is being connected by telegraphic observations with Bonn and Berlin in Germany, and with Geneva and Neufchatel in Switzerland. Two astronomers from Berlin having arrived in Paris, and M. Loewy, member of the French Academy of Sciences, with two assistants, having arrived in Berlin from Paris, the work has been at once proceeded with. The wires are freed a few hours every night for obtaining comparisons. The connection with Geneva and Neufchatel is executed, *via* Lyons, by Commander Perrier, of the staff, and the operations have been continued to Marseilles and Algiers. The comparison between the Montsouris and Paris observatories will be a work of triangulation, the two establishments being about a gun-shot from each other.

A NUMBER of Abyssinians have arrived in Paris on their way to London. They are encamped in the Acclimatisation Gardens (Bois de Boulogne), with camels, elephants, ostriches, &c., and other animals destined to the London Zoological Gardens. The heads and manners of the blacks have been scientifically examined by Dr. Broca, and a report on them will be read at the French Society for the Advancement of Science at Havre.

THE Bureau of the French Association to meet in Havre on the 23rd instant, consists of Prof. Broca, president; M. Kuhl-

mann, vice-president; M. Deherain, general secretary; M. Perrier, vice-secretary; M. Masson, treasurer. Most of the French railway companies give half-price tickets to persons going to the Association. The hotel proprietors in Havre guarantee a certain number of beds; furnished apartments have also been largely promised, and the berths in one of the Transatlantic Company's steamers have been placed gratuitously at the disposal of members.

AN interesting account of the recent falling of a mountain in Tarentaise, Savoy, causing disaster to two flourishing villages, has been communicated to the *Courrier des Alpes*, by M. Bérard. The phenomenon has been incorrectly reported as instantaneous, and the destructive effect complete, whereas the case is that of a mountain which for twenty days, without cessation, has been dismembering itself and literally falling night and day, into the valley below, filling it with piled-up blocks of stone, extinguishing all sounds by its incessant thunder, and covering the distant horizon with a thick cloud of yellowish dust. The entire mass comprised in the slope forms a mutilated cone 200 metres broad at the top and 600 at the base (the slope being about 50°); this is composed of blocks of hard schist lying close together, but no longer united; and it is united to the body of the mountain only by a vertical mass 40 to 50 m. thick, which already is fissured and shaken. Periods of repose occur lasting only a few seconds or a minute at most; then the movement recommences, and continues about 500 hours. Blocks of 40 cubic metres become displaced with no apparent cause, traverse the 1800 m. of descent in thirty seconds, leaping 400 or 500 m. at a time, and finally get dashed to pieces in the bed of the torrent, or launch their shattered fragments into the opposite forest, mowing down gigantic pines as if they were so many thistles. One such block was seen to strike a fine fir-tree before reaching the bridge between the villages; the tree was not simply broken or overthrown, but was crushed to dust (*volatilisé*); trunk and branches disappeared in the air like a burning match. Rocks are hurled together and broken into fragments that are thrown across the valley like swallows in a whirlwind; then follow showers of smaller fragments, and one hears the whistling sound of thousands of pebbles as they pass. M. Bérard reached the edge of the rock (2,460 m. high), on one of the sides of the falling cone, and ventured along it, obtaining a good view of the "terrifying" spectacle. He reaffirms his conviction that the phenomenon is inexplicable by any of the usual reasons that account for Alpine disturbances, such as penetration of water, or melting of snows, or inferior strata in motion; nor does the declivity of the slope explain it. His hypothesis is that some geological force is at work, of which the complex resultant acts obliquely to the axis of the mountain and almost parallel to its sides.

ACCORDING to M. Perrin, an eighth or a tenth portion of the French army is incapable of doing good service, in consequence of indistinct vision. M. Perrin formally proposes to remedy this by the adoption of spectacles. It is affirmed that spectacles are useful, if not indispensable, to 47 per cent. of the officers coming from the École Polytechnique.

FROM the Annual Report of the Council of the Royal Society of New South Wales, we gather that the membership at the beginning of the session of 1877 was 298, and that the Society is in a generally flourishing state. A considerable access of activity has occurred since the establishment of sections (nine) last year. The Council are hopeful of obtaining an annual endowment from the Government.

FOR want of space the gigantic Giffard captive balloon will not be constructed, as was anticipated, in the Paris Exhibition, but special ground will be granted as we announced a few months ago. The

spot selected is the Carrousel interior yard. The large space within the railings has been found sufficient, after special inspection by MM. Lefeul and Tetreau. The ministerial sanction is expected daily. M. Giffard is continuing his experiments on the production of hydrogen gas with continuous apparatus.

A BALLOON was sent up on Wednesday carrying an aëronaut, and elicited an interesting fact of aerial physics. The ground current was blowing gently from north-west, but higher up a south-west current was met by the aëronaut. The balloon was carried at a rate of 500 metres per minute to the north-east of Paris. In the night 8 millimetres of rain fell, the upper current having descended into contact with the ground.

A GERMAN Society for the Exploration of Palestine has recently been started by Dr. Zimmermann, Gymnasial Rector in Basle, along with Professors Kautsch and Socin, of Tübingen. Several other *savants* have joined it. The first quarterly number of the society's projected journal will appear shortly. The annual contribution to the society (10 marks) entitles one to receive the journal.

IT is proposed in Stuttgart to erect a simple monument over the grave of Th. v. Heuglin, the well-known African traveller, recently deceased. The committee, at whose head is Prince Hermann of Saxe-Weimar, invite subscriptions.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus radiatus*) from India, presented by Mr. C. L. Norman; three Chaplin Crows (*Corvus capellanus*) from Persia, presented by Dr. J. Huntley; a West African Python (*Python seba*) from West Africa, presented by Mr. Lionel Hart; a Red River Hog (*Potamochoerus penicillatus*) from West Africa, received in exchange; a Barbary Ape (*Macacus inuus*) from North Africa, a Squirrel Monkey (*Saimaris sciurica*) from Guiana, deposited; a Military Maccaw (*Ara militaris*) from South America, purchased; ten Amherst pheasants (*Thaumalea amherstiae*), three Temminck's Tragopans (*Cerionis temminckii*) bred in the Gardens.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, July 23.—M. Peligot in the chair.—The following papers were read:—New researches on electro-capillary phenomena, by M. Becquerel. One experiment is this: into a cracked tube containing nitrate of silver solution are introduced some very small fragments of carbon, and the tube is put in a vessel holding monosulphuret of sodium. Here the wall of the crack in contact with the inner solution is the negative pole of the electro-capillary couple, and that in contact with the outer solution the positive. Not only does the negative wall get covered with metallic silver, but the carbon fragments are also coated in proportion to their nearness to the crack. Each fragment acts like the crack. The action is like what occurs in a metallic circuit composed of several conductors.—Fixation of nitrogen on organic matter and formation of ozone under the influence of weak electric tensions, by M. Berthelot. He has given up metallic armatures, introducing the gas into an annular space between two vessels holding dilute sulphuric acid solution, which were connected with the battery poles. He mentions four reactions in which formation of ozone has thus been obtained. Again, to estimate fixation of nitrogen, a glass cylinder (with spherical calotte), internally covered with tin, externally half with water-moistened Berzelius paper, half with syrup solution of dextrine, was placed on a lac-covered glass-plate and enclosed in a concentric glass cylinder with outer coating of tin; the tin armatures were connected with five Leclanché elements during several months, and fixation of nitrogen in paper and dextrine was demonstrated. He shows the application of such facts.—On an experiment by Dr. Bastian relating to urine neutralised by potash, by M. Pasteur. He describes a form of Dr. Bastian's experiment he has performed several times in presence of Academy members, and never got bacteria; the nature and treatment of the vessel is a salient point.—Tertiary strata of Hungary (continued), by MM. Hebert and Munier-Chalmas.—On the electric conductivity of trees, by M. Du Moncel. After referring to the local currents

and currents of polarisation got on applying to each tree two platinum electrodes 9 ctm. square, with an interval of 6'44 m., he gives a table of resistances for various species. The soft woods with spongy tissue and vigorous vegetation, such as elm (resistance 1,431 km.), chestnut (1,694), lime (1,988), poplar (2,090), are the best conductors. Among hard woods with slow vegetation, box had a resistance of 12,511 km. Birch (4,777) formed an exception.—Reply to M. Cosson's observations on the Saharan Sea, by M. D'Abbadie. M. de Lesseps corroborates M. D'Abbadie's arguments.—On the ophitic phenomenon in the Pyrenees and the Haute-Garonne, by M. Leymerie. Ophite proper and lherzolite are two different but concomitant facies of an eruptive phenomenon characteristic of the Pyrenees, which may, as a whole, be termed *ophitic*. It is only met with in the lower part of slopes.—Reply to M. Naudin's observations on the interior sea of Sahara, by M. Roudaire.—On the degree of efficacy of sulphide of carbon as a means of destruction of phylloxera, by M. Boiteau.—On the grape-disease of the Narbonne vineyards, by M. Cornu.—On the Doryphora of potatoes, by M. Girard. He thinks sulpho-carbonate of potash would be useful against it; also that the fear of the beetle is exaggerated. Another chrysomelid (*Colaspidea atrum*), which attacks lucern in France, is very like the Colorado beetle in its ways, and it is successfully resisted.—On curves having the same principal normals, and on the surface formed by these normals, by M. Mannheim.—On the extension to space of two laws relative to plane curves, given by M. Chasles, by M. Fouré.—Influence of heat on magnetisation, by M. Gauguin. Certain magnetic bars of Sheffield steel heated and let cool are found at last to have changed in the sign of their magnetism.—On the magnetisation of circular plates where the isodynamic lines are concentric circumferences, by M. Duter.—On the electrolysis of sulphurous acid, by M. Gueront. This substance is decomposed like a salt.—Note on the determination of manganese, nickel, zinc, and lead, by M. Riche.—On the density of vapour of sulphhydrates of ammonia, by M. Horstmann.—On the nature of gases contained in the tissues of fruits, by M. Livache. He applied M. Schlessing's analytic method of immersion in ether (without lesion of tissue). In the tissues of healthy fruit the gases are a mixture of nitrogen and oxygen in the proportions found in air.—On the products of fermentation of the mud of Paris, by M. Maumene.—On the fecundation of the star-fish and sea-urchin, by M. Fol.—On the anatomy and the migrations of oxyurides, parasites of insects of the genus *Blatta*, by M. Ghalab.—Influence of the sun and moon on magnetic and barometric variations, by M. Broun.—Some observations on the trajectory of hail during thunderstorms, by M. Ziegler. A hailstone cannot (he considers) attain a great weight except through a long course in dense air in the lower regions of the atmosphere, and he cites cases to prove that the trajectory of large hailstones forms a very acute angle with the ground.

CONTENTS

| | PAGE |
|--|------|
| THE PHYSICAL BASIS OF MIND. By DOUGLAS A. SPALDING . . . | 261 |
| GORE'S "ELECTRO-METALLURGY" . . . | 263 |
| OUR BOOK SHELF:— | |
| Betz's "Enlucidation de las Plantas Europeas que se hallan como silvestres en la Provincia de Buenos Aires y en Patagonia" . . . | 264 |
| LETTERS TO THE EDITOR:— | |
| Optical Spectroscopy of the Red End of the Solar Spectrum.—Prof. PIAZZI SMYTH, F.R.S. . . . | 264 |
| The Cretaceous Flora of America.—J. S. NEWBERY . . . | 264 |
| Meteorological Notes from Lisbon.—HENRY O. FORBES . . . | 265 |
| Fertilisation of Flowers by Insects.—HERMANN MÜLLER . . . | 265 |
| Local Museums.—J. ROMILLY ALLIEN . . . | 266 |
| Proposed New Museum.—HENRY H. HOWORTH . . . | 266 |
| Adaptation of Plant Structure.—HENRY COLLETT . . . | 266 |
| Rattlesnakes in Wet Weather.—HUNTER NICHOLSON . . . | 266 |
| Meteors.—W. AINSLIE HOLLIS . . . | 266 |
| OUR ASTRONOMICAL COLUMN:— | |
| The Herschel Companion of Aldebaran . . . | 266 |
| The Third Comet of 1759 . . . | 267 |
| METEOROLOGICAL NOTES:— | |
| Sun-spots and Rainfall in Calcutta . . . | 267 |
| Winds of the South Atlantic . . . | 267 |
| Climate of Kossier, on the Red Sea . . . | 268 |
| Drought in Canada . . . | 268 |
| EARLY ALLUSIONS TO THE MAGNETIC NEEDLE | 268 |
| EVOLUTION OF NERVES AND NERVOUS SYSTEMS, II. By GEORGE J. ROMANES, M.A., F.L.S. (With Illustrations) . . . | 269 |
| THE NORWEGIAN ATLANTIC EXPLORING EXPEDITION . . . | 271 |
| MR. FROUDE'S NEW DYNAMOMETER (With Illustrations) . . . | 272 |
| THE COMMISSION OF THE FRENCH ACADEMY AND THE PASTEUR-BASTIAN EXPERIMENTS. By Dr. H. CHARLTON BASTIAN, F.R.S. . . . | 275 |
| NOTES . . . | 279 |
| SOCIETIES AND ACADEMIES . . . | 280 |

ERRATUM.—P. 238, col. 1, line 9 from top, for *Ekdennite* read *Ekdemite*.

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PAGE
261
263

264

264

264

265

265

266

266

266

266

266

267

267

267

268

268

268

269

271

272

276

279

280

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